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ART. XV.—*On the possible Variability of the Earth's Axial Rotation, as investigated by Mr. Glasenapp;* by SIMON NEWCOMB.

IN the number of this Journal for September, 1870, I called attention to the fact that there were apparent inequalities of long period in the motion of the moon, which had not been accounted for by the gravitation of the known bodies of the solar system, and suggested that they must be accounted for in one of three ways: either (1) there are inequalities in the motion of the moon, due to the gravitation of the sun and planets, which have hitherto eluded mathematical computation; or (2) the motion of the moon is affected by the action of some other forces than that of gravitation; or (3) the time of the earth's rotation on its axis, and therefore the length of its sidereal day, is subject to irregular variations of long period. Of these three exhaustive hypotheses the second was shown to be improbable, because such forces would be likely to produce either regular inequalities of short period, or progressive secular variations, instead of the very slow and irregular changes actually observed. If, then, the first hypothesis were excluded, we should have to fall back on the third as the most probable explanation.

The investigation of the first hypothesis is a purely mathematical process, admitting in theory of being carried through with any degree of rigor. Since the publication of the paper in question I have been engaged in this investigation, and although the pressure of other engagements has prevented its completion, it is so far advanced as to make it quite improbable that there are any other inequalities of long period in the motion of the

moon produced by the gravitation of the planets than the one due to the action of Venus, discovered by Hansen. Assuming provisionally that this result is correct, and that the gravitation of the known bodies of our system cannot produce the observed inequalities of long period, we are forced to accept the hypothesis of the variability of the sidereal day as a provisional theory. At the same time, so long as we have no independent proof of such variability, it cannot be accepted as an established fact. It therefore becomes very desirable to find some independent test of the invariability of the sidereal day. In the paper referred to it was remarked that observations of the interior planets, especially transits, might afford such a test. But it has since occurred to me that eclipses of the first satellite of Jupiter might afford a yet better and more decisive test. The definitive and exhaustive application of this test would require the complete re-investigation of both the theory and observations of Jupiter's satellites, a work for which I have collected some of the materials, but which I have not been able even to commence.

There is, however, one circumstance which rendered the satisfactory application of the test very easy. Granting that the inequalities were really to be accounted for by changes in the earth's rotation, the most extraordinary and sudden change of which we have knowledge occurred about 1860. The velocity of rotation, which for the ten or twenty years previous had been rather slower than the average, was then suddenly accelerated, so as to cause a subsequent gain of perhaps a second per annum, which continued at least till 1872. Collecting all the accessible observations of Jupiter's first satellite from 1850 to 1871, a similar change seemed to be indicated by them, but it was only about half as great as that indicated by the moon, and no greater than its possible error; so that the result did not in any considerable degree affect the probability of the hypothesis.

Last summer I learned that Mr. Glasenapp, of the Pulkowa Observatory, was engaged in an extended investigation of recent observations of the satellites of Jupiter; and, as it seemed desirable that the proposed test of the hypothesis should be made by another, I requested Mr. Glasenapp to try whether the times of the eclipses of the first satellite would be better represented when they were corrected for the hypothetical changes in the earth's rotation. Assuming the earth to be correct at the epochs 1840 and 1870, we should have *earth time slow* by the following amounts at certain other epochs:

Year.	s.	Year.	s.	Year.	s.
1850·5	+ 2	1862·5	+11	1868·5	+ 2
1855·5	+ 5	1864·5	+10	1870·5	0
1860·5	+10	1866·5	+ 6	1872·5	− 2

Mr. Glasenapp has just published his paper in the Russian language, the concluding part of which is devoted to this investigation. He has also sent me the following more extended account of his investigation, from which I omit the citation of the original observations.

"A great many preparations for the expedition of the transit of Venus, and the publishing of a Russian memoir on the observations of Jupiter's first satellite, have not allowed me to take up the question of the variability of the earth's axial rotation, which you proposed to me some time ago. But now I have investigated this very interesting question, and the result seems to me to be satisfactory, so that your hypothesis is very probable.

"In the investigation I have chosen the two following ways: (1) I have tried if the corrections of noon:

Year	1845	+ 1ˢ	
	1850	+ 2	
	1855	+ 5	
	1860	+10	
	1862	+11	
	1864	+10	(1)
	1866	+ 6	
	1868	+ 2	
	1870	0	
	1872	− 2	

you sent to me in your letter dated October 24, 1873, applied to the observed times of the eclipses of Jupiter's first satellite, will bring them in better agreement with the tables of Damoiseau than the uncorrected observations. The result I obtained is favorable, that is, the observations corrected by the quantities (1) are represented better than the uncorrected ones: thus your corrections seem to be real.

"(2.) I determined the corrections of Damoiseau's ecliptic tables of the first satellite for 44 different epochs (22 epochs from eclipse disappearing and 22 from eclipse reappearing), and it was evident that these corrections, and consequently the corrections of noon (when the corrections of Damoiseau's tables are considered as constant quantities) change in the same manner as the corrections (1) you obtained from the observations of the moon, and that their periodicity is very near the same you have found.

"Allow me to communicate to you the whole investigation of this interesting problem.

1. *First investigation.*

"All the observations of the eclipses of the first satellite (as disappearing, as reappearing) from 1848 till 1873, which I

could find in astronomical literature, have been reduced to a homogeneous form, that is, they were corrected for the influence of the different dimensions of object glasses, for the influence of Jupiter's different zenith-distances at the time of observation, for the influence of different distances of Jupiter from the earth, and for the influence of all other circumstances which may change the apparent brightness of the satellite, and which can be taken into consideration. Each observation gives an equation of condition of the form:

$$x+\Delta \cdot y+\rho \cdot z+(C-O)=0, \qquad (2)$$

where x is the correction of the ecliptic tables, Δ the distance of Jupiter from the earth, y the correction of the adopted velocity of light ($493^s{\cdot}2$ by Delambre), and $\rho \cdot z$ a correction to be applied to the observations for different distances of the satellite from the center of Jupiter. The member $\rho \cdot z$ must be introduced in the equation of condition, because all the reductions of the observations are made by Bailly's formula * (plus a constant member C), which is probably imperfect.

"The solution of the equations (2) gives:

a. Eclipse disappearing.	*b.* Eclipse reappearing.	
$x_0=-60^s{\cdot}6\pm 8^s{\cdot}0$	$x_1=-9^s{\cdot}0\pm 8^s{\cdot}0$	
$y_0=+10{\cdot}52\pm 1{\cdot}74$	$y_1=-1{\cdot}47\pm 1{\cdot}26$	(3)
$z_0=-1{\cdot}97\pm 3{\cdot}54$	$z_1=+1{\cdot}60\pm 2{\cdot}56$	
$\gamma_0=\pm 9^s{\cdot}89$	$\gamma_1=\pm 9{\cdot}09$	

(γ_0 and γ_1 are the probable errors of an observation whose weight is unity.)

"Then the same observations were corrected by the quantities (1) and discussed in the same manner as the uncorrected observations; the result will enable us to see if the corrections (1) represent better the observations of the eclipses.

"Each corrected observation gives us an equation of condition of the same form as (2):

$$x+\Delta y+\rho \cdot z+(C-O_1)=0, \qquad (4)$$

in which only the last member O_1 has another signification, because the observed times of the eclipses are all corrected by means of the table (1). The solution of these equations give the following results:

a. Eclipse disappearing.	*b.* Eclipse reappearing.	
$x'_0=-42^s{\cdot}3\pm 7{\cdot}9$	$x'_1=-8^s{\cdot}7\pm 6^s{\cdot}0$	
$y'_0=+6{\cdot}83\pm 1{\cdot}72$	$y'_1=-1{\cdot}12\pm 1{\cdot}24$	(5)
$z'_0=-0{\cdot}64\pm 3{\cdot}50$	$z'_1=+1{\cdot}82\pm 2{\cdot}52$	
$\gamma'_0=\pm 9{\cdot}77$	$\gamma'_1=\pm 8{\cdot}97$	

"If you compare the values of the probable errors γ_0, γ_1, γ'_0, γ'_1, in both cases, you will see that by means of the correc-

* Histoire de l'Académie, Paris, MDCCLXXI, p. 580.

tions (1) the observations of the eclipses of Jupiter's first satellite are represented somewhat better than the uncorrected observations. Therefore we can conclude that these corrections may be real, and that your hypothesis on the variability of the earth's axial rotation may be true.

2. *Second Investigation.*

"The second way I choose for the decision of the same question is to determine the corrections of the ecliptical tables of the first satellite for different epochs, and to deduce from these corrections the corresponding ones of mean noon for the same epochs.

"The values of x_0, y_0, z_0 and x_1, y_1, z_1 (3), which were deduced from the observations without the quantities (1) give the following corrections of Damoiseau's tables:

(1.) Correction of the tabular mean longitude of Jupiter's first satellite, ... $= -27^s{\cdot}7 \pm 4^s{\cdot}8$

(2.) Correction of the tabular semi-duration of the eclipse, ... $= -49^s{\cdot}1 \pm 4^s{\cdot}8$

(3.) Correction of Delambre's velocity of light (of the quantity of 493″·2) .... $= +7^s{\cdot}64 \pm 1^s{\cdot}02$

(4.) Value of z, which is negative for disappearances of the satellite, and positive for his reappearance, ... $= 1^s{\cdot}73 \pm 2^s{\cdot}07$

and with these values we correct the moments of eclipses as given in the Nautical Almanac (Damoiseau's tables) and calculate the quantities (C−O)—that is: calculation—observation—for each eclipse observed since 1848.

"From these quantities we derive the following corrections of Damoiseau's ecliptic tables of the first satellite.

a. Eclipse disappearing.			*b.* Eclipse reappearing.		
	C−O	Weight.		C−O	Weight.
1848·86	+ 6·9	4·7	1848·24	−16·7	7·7
49·22	+ 6·7	3·4	49·27	− 4·5	8·5
50·00	+22·7	2·4	50·32	−15·4	7·6
51·10	+ 9·2	5·3	53·50	−30·7	1·9
52·70	+22·2	1·6	55·70	−17·2	2·7
55·50	+33·2	2·0	56·97	−11·5	3·6
56·58	+20·9	4·7	58·10	−12·2	4·5
57·72	+16·3	22·7	59·15	− 5·9	9·7
58·82	+11·5	7·7	60·21	− 6·3	8·1
59·90	+22·9	2·0	61·30	+ 4·4	0·9
61·22	+14·5	3·3	62·32	−12·7	4·4
62·17	+29·0	2·9	63·37	−28·3	3·6
63·15	+22·0	3·5	64·60	−11·5	1·6
64·28	+ 4·7	7·5	65·70	−27·8	0·7
66·45	+22·6	1·8	66·72	−18·8	3·3
67·63	+33·9	2·6	67·82	−16·9	4·9

a. Eclipse disappearing.			*b.* Eclipse reappearing.		
	C—O	Weight.		C—O	Weight.
1868·65	+33·8	2·0	1868·83	−16·5	6·7
69·76	+15·2	12·5	69·45	−23·3	2·0
70·84	+ 8·4	6·3	70·05	−24·2	1·8
71·90	− 2·6	9·6	71·14	−20·6	8·0
72·50	+ 5·3	8·2	72·21	−17·4	21·9
73·25	+15·0	10·2	73·24	−16·5	26·4

"To discuss these corrections by the method of least squares, let us adopt the following form for the equations of condition:

$$x + k(t - t_0) + m(t - t_0)^2 + C - O = 0,$$

where t is the year of observation, $t_0 = 1861{\cdot}00$, x the correction of the tables for 1861·00, k and m the co-efficients to be determined from the observations.

"From these equations we obtain the following normal equations:

a. Eclipse disappearing.

$$\begin{aligned} &+ 126{\cdot}9x + 259{\cdot}3k + 8227m + 1718 = 0 \\ &+ 259{\cdot}3x + 8227\,k + 35467m + 1706 = 0 \\ &+ 8227\,x + 35467\,k + 923395m + 85480 = 0 \end{aligned}$$

b. Eclipse reappearing.

$$\begin{aligned} &+ 140{\cdot}5x + 455{\cdot}6k + 12239m - 2111{\cdot}8 = 0 \\ &+ 455{\cdot}6x + 12239\,k + 55150m - 9823 = 0 \\ &+ 1223{\cdot}9x + 55150\,k + 1558741m - 196478 = 0 \end{aligned}$$

which give for x, k, and m the values:

a. Eclipse disappearing.	*b.* Eclipse reappearing.	
$x_0 = -17{\cdot}76 \quad \pm 1.25$	$x_1 = +12'69 \quad \pm 0{\cdot}86$	
$k_0 = +\ 0{\cdot}08317 \pm 0{\cdot}1553$	$k_1 = +\ 0{\cdot}25118 \pm 0{\cdot}09172$	(7)
$m_0 = +\ 0{\cdot}06245 \pm 0{\cdot}02178$	$m_1 = +\ 0{\cdot}01753 \pm 0{\cdot}01356$	

"If the reduced observations were quite homogeneous, no difference should be found between the quantities k and m, deduced respectively from the disappearances and reappearances of the first satellite; but the difference we have found between the two values of k and m is larger than their probable errors allow it, so that properly we have no right to take the mean of the values k_0 and k_1, m_0 and m_1, and to consider it as the most probable value of m and k. Still we take these means and consider them as the most probable values because we do not know the cause of this discordance, and cannot therefore make any other combination of the values k_0 with k_1, and m_0 with m_1.

"The combined values of x, k and m are

$$\begin{aligned} x &= +3''{\cdot}52 \quad \pm 0{\cdot}71 \\ k &= +\ 0{\cdot}2076 \pm 0{\cdot}0783 \\ m &= +\ 0{\cdot}3006 \pm 0{\cdot}01151 \end{aligned} \qquad (8)$$

"The probable error of k is very large in comparison with its whole value, and this arises from the want of a sufficient number of observations of the eclipses. Indeed, if you consider the corrections of Damoiseau's tables, you can easily see that very few observations are made during many years, and during some not at all.

"By means of the quantities (8) we calculate the following corrections of the tables for the same epochs, for which you obtain the corrections (1):

1850·5	+4·6	
55·5	+3·4	
60·5	+3·4	
62·5	+3·9	
64·5	+4·5	(9)
66·5	+5·5	
68·5	+6·8	
70·5	+8·2	
72·5	+9·9	

And if the corrections of the tables may be considered as constant quantities, the values (9) with opposite signs will be the corrections of noon plus any constant quantity; thus when we change the signs in the quantities (9) and add to each of them the constant +7ˢ·9, so that at 1872·5 the correction of noon shall be the same as you have found (1), we obtain the following values for the correction of noon.

Correction of noon.		Correction by S. Newcomb.
1850·5	+3″	+ 2″
55·5	+4	+ 5
60·5	+5	+10
62·5	+4	+11
64·5	+3	+10
66·5	+2	+ 6
68·5	+1	+ 2
70·5	0	0
72·5	−2	− 2

"The comparison of these series shows:

1. That the change of the corrections is similar in both series.
2. That the periods are very near each other.
3. That the maxima coincide.

"Although the corrections themselves, in both series, differ much from each other, so that they alone cannot give us the right to make any conclusion on their reality, but as the disappearances of the first satellite, as the reappearances (*a*) give the same sign for the values of k and m, (*b*) that the change of the corrections (*g*) is similar in both series, (*c*) that the periods of these changes are very near the same, in both cases, and (*d*)

that the maxima coincide,—it seems to me that we have the whole right to ascribe reality to your remarkable hypothesis on the variability of the earth's axial rotation."

On this paper of Mr. Glasenapp I remark that the case does not seem to me so well made out as he considers it. The hypothesis to be tested is not simply that the rotation of the earth is variable, but that the outstanding errors in the moon's place are due to this variation. The hypothesis can be made more probable only by showing that the eclipses of Jupiter's satellites are better represented when the hypothetical corrections are applied. Now, by some unfortunate concurrence of errors of theory or observation, the results from Jupiter's satellite seem to fall just half way between the two hypotheses, (1) of invariable sidereal day, and (2) of such variable sidereal day as will account for the apparent errors of lunar theory, and therefore leave the question undecided.

The question may, however, arise, whether the corrections of "earth time" already cited are the only ones which will represent the motion of the moon. On this I remark that there is one doubtful term of long period in Hansen's tables of the moon which I did not take out in making the comparison on which the above correction is founded. This term is that depending on the longitude of the moon's node, which Hansen has found more than a second larger than Airy and others have from observations. I shall, therefore, determine the outstanding mean errors in the lunar theory when we take from Hansen's tables; (1) the excess of his secular acceleration over theory, $5''.4T^2$; (2) the empirical term depending on the action of Venus; and (3) the above mentioned excess of his value of the 19 year term, assuming it to be 1″. The following table,

Year.	Correction given by		Concluded Correction	Errors of Hansen's Theory.			Corrections of pure theory.		Earth slow.
	Greenwich.	Washington.		(1.)	(2.)	(3.)	(1.)	(2.)	
	″	″	″	″	″	″	″	″	s
1850	+0·3	−1·3	0·0	+0·05	+0·56	+0·7	+1·3	−0·4	−0·8
51	+1·5	+0·1	+1·3	0·04	0·45	+1·4	+2·8	+1·4	+2·7
52	+0·9	----	+0·9	+0·03	0·36	+1·5	+2·3	+1·2	+2·3
56	+1·0	----	+1·0	0·00	0·09	+0.3	+1·4	+1·5	+2·8
57	+1·5	----	+1·5	----	0·02	−0·1	+1·4	+1·8	+3·4
58	+2·0	+1·5	+1·8	----	0·05	−0·4	+1·4	+2·1	+4·0
62	+2·4	+2·4	+2·4	----	0·05	−1·0	+1·4	+3·3	+6·3
63	+2·2	+1·2	+1·7	0.00	0·02	−0·8	+0·9	+3·1	+5·9
64	+0·1	−1·0	−0·4	+0·01	0·09	−0.6	−0·9	+1·6	+3·0
65	−1·1	−2·4	−1·7	0·01	0·14	−0·3	−1·5	+1·3	+2·5
66	−2·2	−2·5	−2·4	0·02	0·20	0·0	−2·2	+0·9	+1·7
67	−3·9	−4·1	−4·0	0·03	0·28	+0·3	−3·4	0·0	0·0
68	−4·4	−4·5	−4·5	0·03	0·36	+0·6	−3·5	+0·2	+0·4
69	−4·6	−5·5	−5·0	0·04	0·45	+0·8	−3·7	+0·3	+0·6
70	−5·0	−6·1	−5·5	0·05	0·56	+0·9	−4·0	+0·3	+0·6
71	−7·0	−7·2	−7·1	0·06	0·67	+1·0	−5·4	−0·8	−1·5
72		−7·8	−7·8	+0·07	+0·80	+1·0	−5·9	−1·0	−1·9

partly taken from my former paper, shows the correction of Hansen's tables given by the Greenwich and Washington observations, and the modification of the theory just mentioned. In the second column of "corrections to pure theory," the theoretical annual motion of the moon is supposed to be diminished by $0''.30$.

The difference between the numbers in the last column and those formerly cited arises almost entirely from the change of $1''$ in the value of Hansen's nineteen-year coefficient. Let us now treat Mr. Glasenapp's residuals by a method intermediate between the two which he adopts. I have divided his residuals into ten groups, and in order to make the observations of ingress and egress comparable, have corrected them as follows:

The ingress residuals by $-15^s.2+0^s.08\ (t-1861.0)$
The egress residuals by $+13.4+0.25\ (t-1861.0)$

and have then taken the mean by weights of each group. The results are as follows:

Excess of Theory over observation.

Ingress.			Egress.			Combined.			
							C–O.		
Date.	C–O.	Wt.	Date.	C–O.	Wt.	Date.	Uncorrect'd	Corrected.	Wt.
	s.			s.			s.	s.	
1849·0	− 9·4	8	1848·8	+ 0·1	16	1848·9	−3·1	−2·1	24
50·7	− 2·6	8	50·3	− 4·7	8	50·5	−3·6	−3·6	16
52·7	+ 6·2	2	53·5	−19·2	2	53·1	−6·5	−9·0	4
56·3	+ 9·0	7	56·4	− 1·9	6	56·3	+3·9	+0·9	13
58·5	− 0·4	30	58·8	+ 4·9	14	58·6	+1·3	−2·7	44
61·1	+ 6·7	8	61·0	+ 5·7	13	61·1	+6·1	+0·1	21
63·7	− 1·1	16	63·7	− 8·8	5	63·7	−3·0	−8·5	21
67·6	+15·1	6	67·8	− 2·5	16	67·8	+2·3	+1·7	22
70·5	− 4·0	9	70·7	− 5·9	12	70·6	−5·2	−5·2	21
72·6	− 8·1	28	72·8	− 0·6	48	72·7	−3·4	−1·4	76

The columns C–O show the residuals, (1) when the length of the day is supposed uniform; (2) when the times are corrected for hypothetical "earth slow" from the last column of the last table. We have now to see which series can be best represented by an expression increasing uniformly with the time. Solving by least squares we find the expressions to be:

Uncorrected residuals $= -0^s.80 - 0^s.100(t-1860.0)$
Corrected residuals $= -2.45 + 0.039(t-1860.0)$.

Subtracting the value of these expressions, we find the residuals still outstanding to be as follows:

Uncorrected.	Corrected.	Wt.
$-3^s{\cdot}4$	$+0^s{\cdot}8$	24
$-3\ {\cdot}7$	$-0\ {\cdot}8$	16
$-6\ {\cdot}4$	$-6\ {\cdot}3$	4
$+4\ {\cdot}3$	$+3\ {\cdot}5$	13
$+2\ {\cdot}0$	$-0\ {\cdot}2$	44
$+7\ {\cdot}0$	$+2\ {\cdot}5$	21
$-1\ {\cdot}8$	$-6\ {\cdot}2$	21
$+3\ {\cdot}9$	$-3\ {\cdot}8$	22
$-3\ {\cdot}3$	$-3\ {\cdot}2$	21
$-1\ {\cdot}2$	$+0\ {\cdot}6$	76

It will be seen that of the ten residuals, all but one are diminished by the application of the hypothetical corrections. Although the observations are too uncertain, and the residuals too irregular, to regard this result as proving the hypothesis, yet it seems to me to render it worthy of reception as being, in the present state of our knowledge, the most probable explanation of the outstanding differences of long period between the theoretical and observed longitude of the moon.

Art. XVI.—*Researches in Acoustics;* by Alfred M. Mayer. Paper No. 5.

(Continued from page 109.)

5. *Six Experimental Methods of Sonorous Analysis described and discussed.*

The remarkable discoveries in sound made in these later times by Helmholtz were owing, in great part, to his having early seen the necessity of obtaining precise knowledge of the composition of sounds,—by means of the methods of sonorous analysis which he had devised,—before one could attempt to give an explanation of the causes of timbre, of the mechanism of audition, and of the physiological causes of musical harmony; and furthermore, he reduced all of the analytic methods and explanations, contained in his classical work, to a harmonious system, by showing how they naturally flowed from the fertile theorem of Fourier. As Helmholtz distinctly states: "In letzter Instanz ist also der Grund der von Pythagoras aufgefundenen rationellen Verhältnisse in dem Satze von Fourier zu finden, und in gewissem Sinne ist diese Satz als die Urquelle des Generalbasses zu betrachten." (Tonempfindungen, p. 346.)

The evident importance of the subject of sonorous analysis will probably render interesting the few remarks I here venture to offer on this subject. I will describe in order six meth-

ods of sonorous analysis. Methods 1, 2 and 5 have been used by Helmholtz and König. Methods 3, 4 and 6, as far as I know, originated with me. To render comparable all of the experiments which I shall describe, I shall always use one composite sound of uniform intensity by sounding with a blast of constant pressure an Ut_2 Grenié free-reed pipe, from which has been removed its reinforcing pyramidal pipe.

(1.) *Analysis by means of Resonators applied directly to the ear.*

This method of Helmholtz is so well known that it need not here be described, but I will give some experiments which I have made on its degree of precision under the head of the sixth method of analysis, to be described.

(2.) *Analysis by means of Resonators connected with König's manometric flames.*

This is the least delicate and accurate of all methods of sonorous analysis, but it has a value in giving an objective, ocular illustration, which is sometimes of use. I do not, however, here refer to the use of a manometric flame placed in connection with a conical pipe, into which one sings, and which instrument in the hands of König has done such admirable work in the analysis of the vowel sounds; but I refer to the harmonic series of resonators connected with as many flames which burn near a long revolving mirror.

The following experiments will show the degree of delicacy of the above apparatus. I sounded the reed pipe with its maximum intensity when it was about one foot from the center of the series of resonators, and produced well marked serrations in the Ut_3 and Ut_4 flames, but the other flames showed only slightly indented top borders. It was only when I sang loudly Ut_3 or sounded this same note on a French horn that the edges of the flames became deeply serrated.

(3.) *Analysis by means of Resonators which are successively brought near the source of origin of a composite sound and thus successively reinforce all of its sonorous elements.*

If we take any two resonators separated by a known interval, and vibrate before them the forks of this interval, we can carry these sounds in the memory. Now close the nipples of the resonators with wax and successively bring their mouths near the origin of the composite sound. If the simple sounds to which the resonators are tuned exist as components of the sound, we shall hear them singing out clearly above the general chorus of the other harmonics. Thus I have often successfully shown to an audience the composition of a composite sound.

(4.) *Analysis by means of Resonant boxes carrying solid bodies tuned in unison with the sounds to which the air in these boxes resounds.*

This method is an excellent one when the composite sound can be obtained with intensity, when the boxes are accurately in tune with the solid bodies (forks or strings) attached to them, and when the boxes are in unison with the sonorous elements of the composite sound which we would analyze. The last mentioned condition is not always fulfilled in the boxes on which forks have been sometime mounted, for the former are apt to change their interior capacity by warping. This fact can readily be ascertained by partly introducing the hand into the mouth of the box, and noting the effect on the intensity of the sound.

This method of analysis is similar to the one previously described; for the resonant box of a fork acts like a resonator and can be used to intensify any harmonic of a composite sound; but there is an important difference in the methods, for the fork or the string being in unison with the proper note of the mass of air in the box, is set in vibration by the latter, so that after the box has been removed from the vicinity of the origin of the composite sound, and the latter has ceased, we find that the fork sings out alone, and thus shows that it has selected from a chorus of harmonics that one which is in unison with its own tone. I have thus been able, by placing one fork after another, of the series of the harmonics of the Ut_2 reed, to show the composition of its sound to a large audience with entire satisfaction. I have also succeeded with the following experiment. Forcibly sound the reed, and place around the opening of its "stump" all of the eight forks, of the harmonic series of Ut_2, with the mouths of their resonant boxes toward the reed. After the reed has sounded for a few seconds, stop it, and we shall find that all of the forks are in vibration; and thus singing together, they approximately reproduce the sound of the reed. This experiment to succeed requires the resonant boxes, the forks, and the harmonics of the reed to be in exquisite unison.

(5.) *Analysis by means of the beating of simple sounds of known pitch with the harmonics of the composite sound to be analyzed.*

If we use forks for the simple sounds, it will be better slightly to flatten or sharpen the note of the sound to be analyzed. Then knowing the number of beats that the fundamental of the note gives with its corresponding fork, we can designate the number of any other harmonic by the number of beats it makes with a fork of known pitch; for the number of beats observed, when referred to the number of beats of the fundamental as unity, will be directly as the number of the harmonic

in the series. If we cannot well alter the pitch of the composite sound, then it will be well to use forks with sliding weights on graduated prongs, or loaded strings accurately tuned and provided with meter scales. In this system of analysis it is very important to guard against being led astray by the beating of resultant tones; therefore, the forks or strings should be gently vibrated and resonators used to assist the ear.

(6.) *Analysis by means of a loose membrane which receives the composite sonorous wave and transmits its vibrations through filaments or light rods to a series of forks mounted on their resonant cases.*

This method of analysis is the one we devised in our experimental confirmation of Fourier's theorem and described in sec. 1 (pp. 82–84). We here wish to call attention to the precision of this method of analysis, while at the same time acknowledging the delicate instrumental conditions required to use it successfully. The principal interest attached to it is that it shows in a vivid manner the operation of the instantaneous decomposition of a composite wave into its elementary pendulum-vibrations. The method has also peculiar interest as showing, in the most striking manner, the exaltation of the action of very feeble periodic impulses to such degrees of intensity as to set into synchronous vibration very large masses of matter; and it may be well before we discuss the subject proper of this section to call attention to this very interesting result, as set forth in the following experiment with our apparatus. To the membrane, covering the hole in the box of the reed-pipe, I attached one end of a fiber of silk-worm cocoon, one meter long and weighing one milligram. The other end of the fiber was cemented to the face of a prong of an Ut_2 fork. This fork weighed 1,500 grams, while the top of its resonant box and the air in the latter weighed respectively 102 and 22 grams. Therefore, the fiber set in motion 1,624,000 times its own weight by only a fraction of the force which traversed it, and even this force was a yet more minute fraction of the whole energy of the aerial vibrations produced by the reed.

An experiment like the above is instructive as an analogical illustration of the manner in which we may imagine an ætherial vibration to produce chemical decomposition by causing such powerful synchronous vibrations in the molecules of compounds as to shake their atoms asunder; and we have already seen how very feeble impulses sent through a medium of great tenuity can, when rapidly recurring and of the proper period, produce mechanical effects which at first sight appear incredible. Time is required in both cases to produce an appreciable action. The time required in the case of the sonorous vibrations de-

creases as their number per second increases, and in the case of the ætherial vibrations we have analogical phenomena. In the acoustical experiment, if the fork be much out of tune with the pulses transmitted by the fiber, no motion is produced in the fork; likewise, we may imagine that when the period of vibration of one or more of the constituent atoms of a certain molecule is far removed from unison with any of the ætherial vibrations falling upon it, no motion, or chemical decomposition, will ensue.

The analogy between the two classes of phenomena is yet more striking when we remember that the fork *selects*, from the composite vibratory motion which traverses the fiber, only that vibration which is in unison (or only slightly removed from unison) with its own proper periodic motion; so, likewise the molecule, or atoms of the molecule, select only those vibrations from the ray which are in unison with their own atomic periods; and on the tuning of the atom depends whether the result of the action of the ray will evince itself as heat, phosphorescence, chemical action, or fluorescence.

The following experiments show that the method of analysis we are now discussing surpasses in delicacy and sharpness of definition any other method in which sympathetically vibrating bodies are employed. As already shown, the forks select from the composite vibratory motion which strikes them only those simple vibrations which are in unison with their own vibratory periods. This remark, however, requires some modification, though the qualification necessary is less than is required when other similar methods of analysis are used. In all cases of co-vibration there is a certain range of pitch, above and below the sound which is in unison with the existing vibration, through which the co-vibrating body responds. The farther the remove from unison the weaker the response.* But in some cases a slight remove of the pitch from unison will cause a great diminution in the intensity of the co-vibrations, while in others the same departure from unison causes only a slight or even inappreciable change in their intensity. For example, I connected the Ut_3 fork—the second in the harmonic series of Ut_2—to the membrane on the Ut_2 reed, and sounded the latter during a few seconds. After the reed was silent, I heard the fork sounding with intensity.† But, on loading the fork with a

* See Helmholtz's Tonempfindungen, p. 217, for the law connecting the variation of intensity of co-vibration with the variation of pitch in the existing body.

† It is perhaps hardly necessary to state that in all of these experiments I first ascertained that when the fiber was detached from the fork, the latter did not emit a sound caused by the action of the intervening vibrating air. This condition is readily attained by screening the mouths of the resonant boxes, or by turning these mouths away from the sounding reed. Also, I should here state that the reed was tuned to the fork after the fiber was stretched between the latter and the membrane on the reed-pipe.

piece of wax, so that it gave *five* beats per second with the note of the fork when unloaded, I could not, by any variation in the tension of the fiber or of any other circumstances of the experiment, set in vibration the fork by means of the pulses sent from the reed through the fiber; yet, on placing the nipple of an Ut_3 resonator in my ear, I perceived that this flattened note of the fork produced a decided resonance, thus showing that although the fork could not respond to its own note flattened five beats per second, yet the resonator, under the same circumstances, did enter into sympathetic vibration. When the fork gave *four* beats per second it responded to the reed, but this response was only audible on placing the ear close to the mouth of the fork's resonant box. With *three* beats per second the sound of the fork was readily perceptible, while the resonator reinforced it very decidedly. When the fork was out of unison *two* beats per second, its sound was slightly increased; and with a departure of *one* beat per second, the response of the fork was yet stronger, but greatly inferior in intensity to that produced when the fork was in unison with its proper sound—the second harmonic of the reed; yet the resonator reinforces this flattened sound as forcibly as it does that which emanates from the unloaded fork. These facts concerning the want of sharpness in the detection of pitch by means of resonators are not in accordance with the statements made in recent popular works on sound, where the resonator is described as remaining dumb until the exact pitch to which it is tuned is reached, when it responds with a suddenness which has been compared to an explosion!

The fork, the stretched fiber and the intensity of the sound of the reed remaining in the same conditions as in the above experiments, I gradually unloaded the fork until it made only *one beat in eight seconds*, and yet even this slight departure from unison with the second harmonic of the reed was evident in the difference in the intensities of the fork's responses when thus loaded and when the wax was removed. This fact I have repeatedly confirmed by testing the intensities of the two sounds by different hearers, who were placed so that they could not see when the fork was loaded or unloaded. Now E. H. Weber has found that only the most accomplished musical ears can distinguish between the pitch of two notes whose vibrations are as 1000 to 1001, but by the above method we can readily detect a departure from unison in the two notes amounting to the interval of 2000 to 2001, or to the $\frac{1}{12}$th of a semitone.

In connection with the preceding observations, the following experiments on resonators and sympathetic vibrations may be of interest. I substituted for the ear the manometric flames of König, viewed in a revolving mirror, and tested the response of

the resonators to sounds not in accord with their proper notes. The results agreed with those previously obtained on placing the resonator to the ear. I now mounted an Ut_3 fork on its resonant case, and sounding it *strongly* before all the resonators of the harmonic series of Ut_2, I caused all of the manometric flames connected with these resonators to vibrate, each giving the same number of serrations as when the Ut_3 fork was brought near its own resonator. The same result was obtained when the fork was separated from its case, with this important difference, however, that when the face of fork Ut_3 was brought near the mouth of the Ut_4 resonator, the flame connected with this resonator gave at the same time serrations belonging to both Ut_3 and to Ut_4, and this result accords with the following experiment. If one sounds the Ut_3 fork on its box, and brings the mouth of the latter near the mouth of the box of the Ut_4 fork, the Ut_4 fork will co-vibrate, and after Ut_3 has ceased to vibrate, Ut_4 will sound out quite clearly. If, however, Ut_4 be lowered in tone, by weighting it with a piece of wax, so that it gives two beats per second with its proper tone, then the Ut_4 fork cannot be set into sympathetic vibration by impulses from Ut_3. Also, if forks Ut_3 and Ut_4 be detached from their boxes, and fork Ut_3 be strongly vibrated while the face of one of its prongs remains quite close and parallel to the face of a prong of fork Ut_4, the latter is set in vibration by the impulses of Ut_3 sent through the intervening air. Fork Ut_3 was now loaded so that it successively made 1, 2, 3, and 4 beats per second with the true Ut_3 pitch. When it made 3 beats per second, it caused Ut_4 to vibrate so feebly as to be barely audible, while 4 beats per second departure of Ut_3 from unison produced no effect on Ut_4.

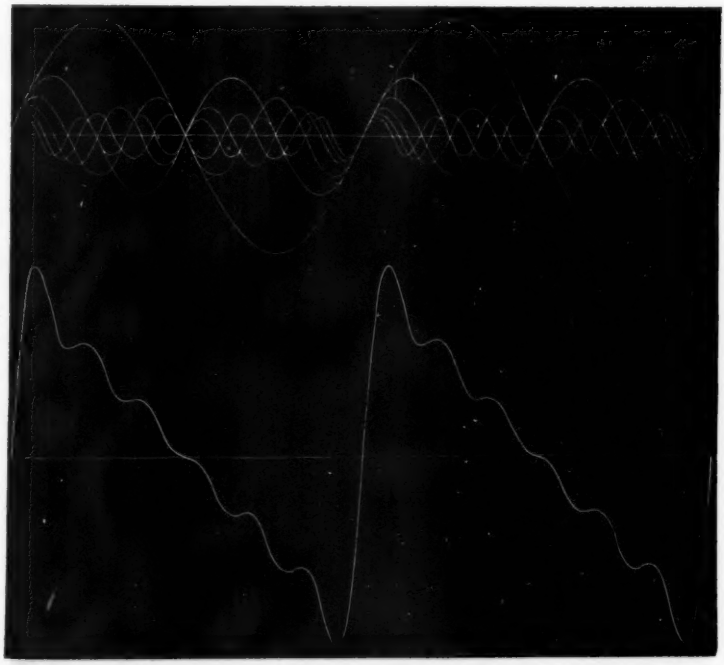
The following experiment was now made to show the want of precision in the determination of the exact pitch of sonorous elements by means of resonators. We have just seen that the Ut_3 fork could not vibrate the Ut_4 fork when both forks were on their cases, and when Ut_3 was flattened by two beats per second; and also that a departure of four beats per second prevented Ut_3 from setting Ut_4 in sympathetic vibration, when both forks were off their resonant cases, and with their prongs close and parallel to each other. But, when the Ut_3 fork was loaded so that it made from 15 to 20 beats with its proper tone, it caused the serrations of the Ut_3 resonator to appear, accompanied by the serrations of its octave. The Mi_3 fork, although it developed the serrations belonging to its own pitch when brought opposite the Ut_3 resonator, yet did not,—as might have been expected,—develop its own tone and the octave when brought near the mouth of the Ut_4 resonator. It is probably not necessary to add that the above effects of simul-

taneously developing in the flame the serrations of the proper note of a resonator and those of its octave are only produced when the fundamental sound is intense.

(7.) ***The Curve of a Musical Note, formed by combining the sinusoids of its first six harmonics; and the curves formed by combining the curves of musical notes corresponding to various consonant intervals.***

We have already seen that any composite vibration, which produces in us the sensation of a musical note, can always be reproduced by the simultaneous production of a certain number of the simple sounds of a harmonic series, provided these simple sounds have the proper relative intensities. Therefore to obtain the resultant curve corresponding to a musical note, we draw on one axis its harmonic components with their proper wave-lengths and amplitudes, and the algebraic sums of their corresponding ordinates are the ordinates of the required resultant curve.

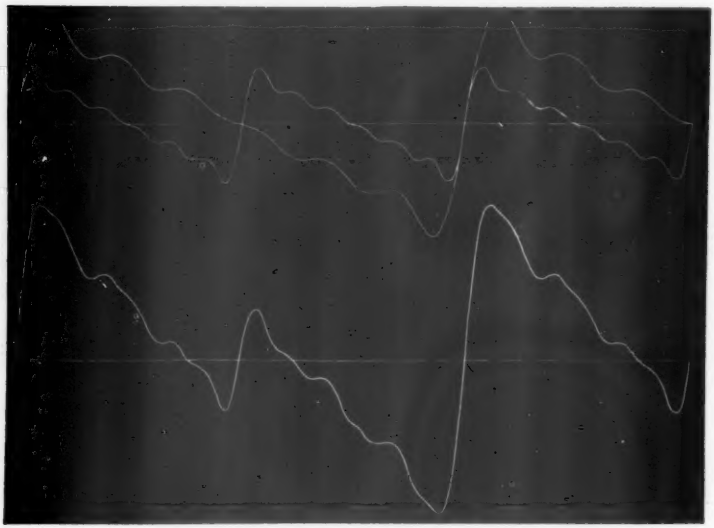
3.



Curve of a Musical Note; being the resultant of the simple vibrations of its first six harmonics.

Fig. 3 is the curve of a musical note, being the resultant of the simple vibrations of its first six harmonics. The first six harmonics having been drawn on a common axis, I erected 500 equidistant ordinates, and extended these ordinates some distance below the axis on which I desired to construct the resultant. The algebraic sum of the ordinates, passing through the harmonic curves, were transferred to the corresponding ordinates of the lower axis, and by drawing a continuous line through these points, I formed the resultant curve. The first six harmonics are alone used in the combinations which I have given, because the 7th, 9th, 11th harmonics, and the major number of those above the 12th, form dissonant combinations with the lower and more powerful harmonics. Indeed, the harmonics above the 6th are purposely eliminated from the notes of the piano by striking the string in the neighborhood of its 7th nodal point. The amplitudes of the harmonics of fig. 3 are made to vary as the wave-length; not that this variation represents the general relative intensities in such a composite sound, but they were so made to bring out strongly the characteristic flexures of the resultant. To simplify the consideration of the curves, they are all represented with the same phase of initial vibration. Of course the resultants have an infinite variety of form, depending on the difference in the initial phase, and on the amplitudes of the harmonic elements.

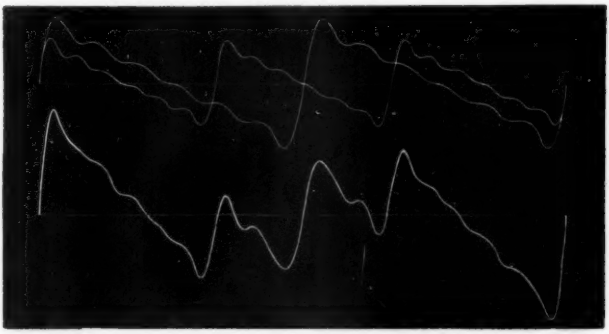
4.



Resultant curve of a musical note combined with its octave. $\lambda : \lambda' :: 1 : \frac{1}{2}$.

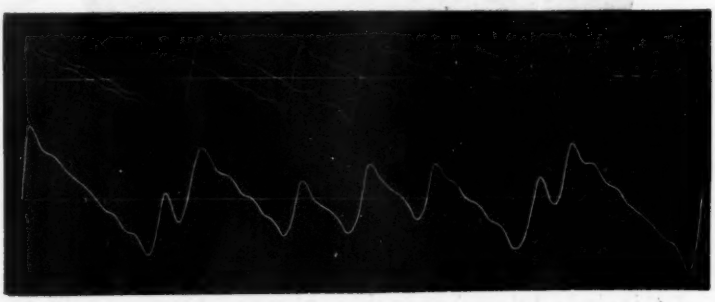
In figs. 4, 5 and 6, I have drawn the resultant curves formed by combining the curves of musical notes corresponding to the various consonant intervals indicated below the curves. As these curves are the resultants formed by the combination of the composite vibrations of musical notes, it follows that the components of these curves are not simple harmonics, as in the case of fig. 3, but are derived from the *resultant* of fig. 3, by reducing to one-fourth the amplitude of that curve and by taking wave-lengths corresponding to the intervals indicated below the figures.

5.



Resultant curve of a musical note combined with its fifth. $\lambda : \lambda' :: 1 : \frac{2}{3}$.

6.



Resultant curve of a musical note combined with its major third. $\lambda : \lambda' :: 1 : \frac{4}{5}$.

All of the curves which I have given in this paper were first drawn on a large scale and then reduced by photography to a size suitable to be transferred to the engraver's block.

(8.) ***Experiments in which are produced from the above curves (sec. 6) the Motions of a Molecule of Air when it is animated with the resultant action of the six elementary vibrations forming a musical note; or is set in motion by the combined action of sonorous vibrations forming various musical intervals.***

We may imagine the curve corresponding to a musical note, represented in fig. 3, as formed by the trace of a vibrating molecule of air, or of a point of the tympanic membrane, on a surface which moves near these points. Therefore if we slide this curve along its axis, under a slit in a screen which allows only one point of the curve to appear at once, we shall reproduce in this slit the vibratory motion of the aerial molecule and

7.

of the point on the tympanic membrane. I have exhibited this motion in a continuous, or rather, recurring manner, as follows: On a piece of Bristol board I drew a circle, and in one quadrant of this circle I drew 500 equidistant radii. On these radii, as ordinates, I transferred the corresponding values of the same ordinates of the resultant of fig. 3, diminished to one-fourth of their lengths. I thus deflected the axis of curve fig. 3 into one-fourth of a circle curve; and this repeated four times on the Bristol board, rendered the curve continuous and four times recurring, as shown in fig. 7. I now cut this curved figure out

of the board and used it as a template. I placed the latter centered on a glass disc of 20 inches in diameter. The disc was covered on one side with opaque, black varnish, and with the template and the separated points of a pair of spring-dividers, I removed from the glass disc a sinuous band, as shown in fig. 7. The glass disc was now mounted on a horizontal axis and placed in front of a lantern the diameter of whose condensing lens was somewhat greater than the amplitude of the curve. The image of that portion of the curve which was in front of the condenser was now projected on a screen, and then a piece of card board having a narrow slit cut in it was placed close to the disc, with the slit in the direction of one of its radii. On now revolving the disc I reproduced on the screen the vibratory motion of a molecule of air, or of a point on the tympanic membrane, when these are acted on by the joint impulses of the first six harmonic or pendulum vibrations, forming a musical note. On slowly rotating the disc one can readily follow the compound vibratory motion of the spot of light; but on a rapid revolution of the disc, persistence of visual impressions causes the spot to appear lengthened into a band; but this band is not equally illuminated—it has six distinct bright spots in it, beautifully showing the six inflections in the curve.

By sticking a pin in the center of fig. 7, as an axis about which revolves a piece of paper with a fine slit, the reader can gain some idea of the complex motion which I have described.

From the curves of figures 4, 5 and 6 can similarly be reproduced their generating motions. Of course it is understood that in all these cases the amplitude of these vibrations are enormously magnified when compared with the wave-lengths, and that it is really only when the amplitudes of the elementary pendulum vibrations are infinitely small that the resultant curves we have given can be rigorously taken as representing what they purport to; for the law of the "superposition of displacements" depends on the condition that the force with which a molecule returns to its position of equilibrium is directly proportional to the amount of displacement, and this condition only exists in the case of infinitely small displacements; yet the law holds good for the majority of the phenomena of sound.

As a periodic vibration can alone produce in the ear the sensation of sound, and as the duration of the period is always equal to the least common multiple of the periods of the pendulum vibrations of the components, it follows that in the case of a simple sound, or of a sound formed of a harmonic series, that the period equals the time of one vibration of the fundamental; but in the case of any other combinations the duration of the period of the recurring vibration increases with the complexity of the ratio of the times of vibration of the compo-

nents; thus, the durations of the following combinations are placed after them in fractions of a second.

$C_3+C_4=\frac{1}{25}$; $C_3+G_3=\frac{1}{128}$; $C_3+E_3=\frac{1}{64}$; $C_3+E_3+G_3=\frac{1}{32}$; $C_3+E_3+G_3+C_4=\frac{1}{77}$th of a second.

The above mentioned facts suggest a curious physiological inquiry, viz: Does it require a combination of sounds, simple or composite, to remain on the ear the duration of an entire period, in order that it shall give the same sensation as is produced when the same combination is sounded continuously? In other words, will a portion of the recurring composite vibration produce the same sensation as an entire period or several periods? The solution of this problem has been the object of a prolonged experimental research, but up to this time the results have been so difficult of interpretation that I have not arrived at any certain knowledge on the subject. I shall, however, return to this interesting but very difficult work.

Art. XVII.—*Description of a new Apparatus for Gas Analysis;* by C. W. Hinman.

Some time since, having occasion to make some analyses of illuminating gas, I read descriptions of several forms of apparatus for that purpose, but failed to find one which appeared quite satisfactory.

An apparatus was desired which should be as far as possible free from fragile or costly parts, and which, without being too complicated, should require no corrections to be made for variations in the pressure, temperature, or aqueous vapor; reliable results rather than minute accuracy being desired.

The apparatus finally adopted operates on the same principles as Williamson & Russell's,* except that in my apparatus the gas is not exploded in the measuring tube, but in a bulb for that special purpose. The trough is nearly the same as that of Doyère,† and pipettes are also used.

The apparatus as made consists of a measuring tube *a*, about 230 mm. long, about 20 mm. in diameter, divided into fortieths of an inch and calibrated with mercury as described by Bunsen. The tube is firmly held by a clamp on the end of the rod *b*, which rod slides up and down in *c*, and is clamped in any position by the screw *d*. A slow motion is given to *c*, and thus to the measuring tube, by means of the milled headed nut *e*, which works along a thread cut on the rod *f*, which is firmly secured

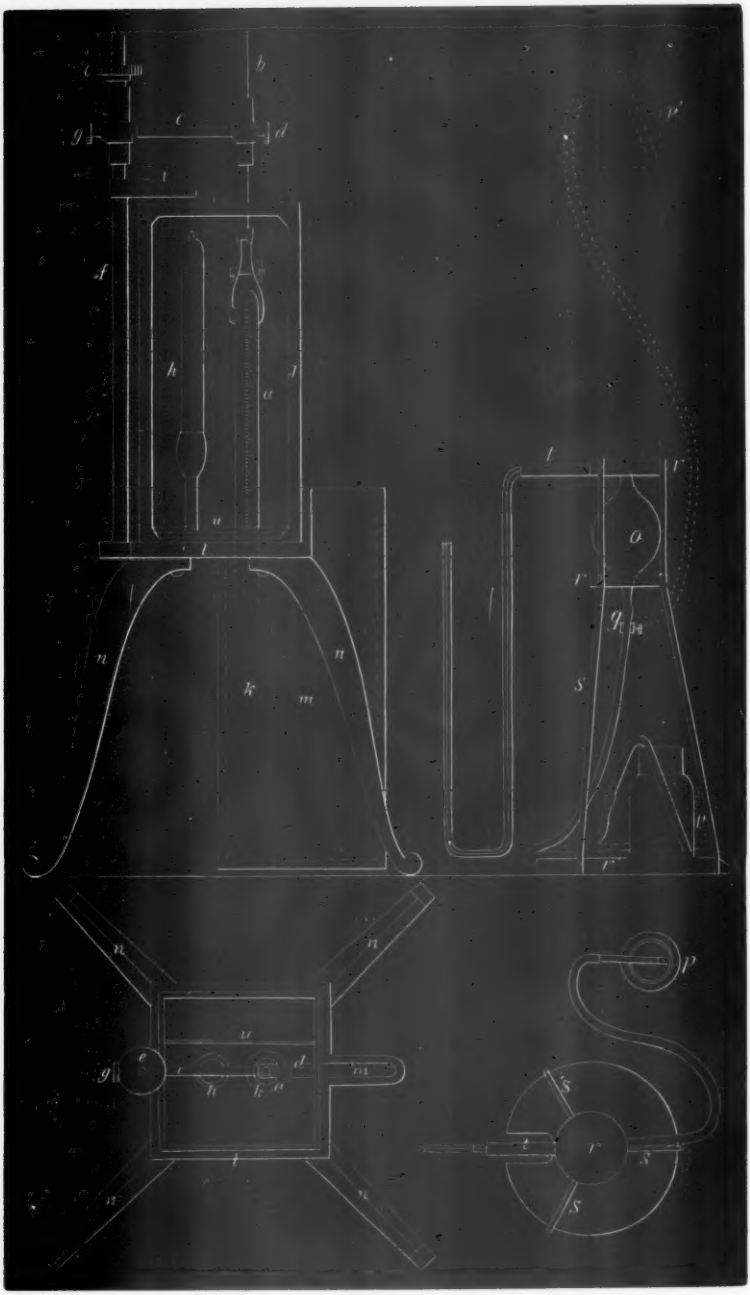
* Chem. Soc. J., [2] ii, 238.

† For figures and description of Doyère's Eudiometer, see Ad. Wurtz Dictionnaire de Chemie, I, p. 280, fig. 42.

to the body of the apparatus. The screw *g* can be turned in so that its end just fits into a longitudinal slot in the rod *f*. By this means *c* is prevented from turning around *f*, and the measuring tube can thus be kept exactly over its well: or *g* can be used to clamp *c* in any position. The pressure tube *h* is about 200 mm. long, and has the same diameter in the upper part as the measuring tube: the lower part, which is about 50 mm. long, has a diameter of only 6 mm. A mass of lead, covered with sealing wax, is attached to the lower part of *h*, so that when it is filled with air, it will keep upright when it is suspended from an eye at the top. The pressure tube is hung by a wire from the end of the arm *i*, which slides with friction along the rod *f*.* The mercurial trough is of cast iron, and consists of the plate *l*, which is about 130 mm. square, 15 mm. thick, and has a groove in its upper surface 10 mm. wide, 6 mm. deep, and 5 mm. from the edge; the well *k*, which is about 240 mm. deep and 30 mm. in inside diameter; and the side well *m*, for the pipette, which is of the same depth as *k*; but part of it extends 40 mm. above the top of *l*; *m* is 10 mm. wide and 90 mm. broad. The wells *m* and *k* have walls 6 mm. thick. Of course *k*, *l* and *m* are all cast in one piece. The groove in *l* has cemented into it, by means of a cement composed of beeswax and rosin, a rectangular trough of plate glass 5 mm. thick, cemented with the same cement into a sheet-iron frame *j*, which is made by bending a sheet of the right size into a box without ends, and then cutting out the sides so as to leave only a strip 12 mm. wide on each edge. The legs *n* are screwed on to the plate *l*.

The pipette consists of a bulb *o*, of a little larger capacity than the measuring tube, open at one end and at the other melted to a tube about 5 mm. in diameter, having a bore of 1 mm., bent so that when it is put into the side well *m*, the point of the pipette can be brought directly under the measuring tube. The other end of the bulb is joined to the end of a piece of good rubber tubing, about 300 mm. long, with rather thick walls and small bore, the other end of which is connected with a small syphon which dips to the bottom of a strong glass bottle *p*, of rather larger capacity than the bulb *o*. Immediately below the bulb is a screw clip *q*. The pipette is held in a stand devised by my assistant, Mr. Lewis. It consists of two discs of wood *rr* (cut away so as to fit the bulb), to which are screwed three rods of iron *ss*, so as to hold the bulb between the discs and at the same time form a tripod to support the pipette. The pipette tube is supported by being attached to a semi-cylindrical piece of metal *t*, which is fastened into the upper disc *r*. The feet of the tripod are screwed into a disc *r'*. This style of

* Compare Dr. Wolcott Gibbs, On a simple method of avoiding observations of temperature and pressure in Gas Analysis, this Journal, II, xlix, 376, 1869.—Eds.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pipette is to hold reagents for absorption. I have invented another sort of pipette in which to explode gases. The bulb is of the same capacity as the former one, but the sides are 12 to 15 mm. thick, and instead of the pipette tube being melted on to the bulb, it is ground into the top with emery and then cemented in with shellac containing a little Venice turpentine. Two grooves are made in opposite sides of the end of the pipette tube where it is ground into the bulb, in which are platinum wires, the ends of which approach within 1 mm. of each other. Care is taken to have the form of the pipette such that all the gas will be expelled from the bulb when the mercury rises.

The apparatus is used in the following manner: Clean mercury is poured into the well until it is about 8 mm. deep above the top of *l*. A drop of water is put in the end of the pressure tube, which is suspended, so that the bottom nearly touches *l*, and water of the temperature of the room is poured in until the glass vessel is nearly full. Air is then taken out of or added to the pressure tube until the mercury in the lower part of the tube is just on a level with the top of a straight-edged piece of glass *u*, which is fixed just behind the pressure tube and measuring tube, so that its top edge is parallel to and about 15 mm. above the top of *l*. The measuring tube being thoroughly cleansed, and a drop of water spread over the sides, is fixed in the clamp; a thin slice of cork being placed between each side of the clamp and the tube. The tube is then placed upright in a small, long-handled cup of mercury, like that used by Doyère, and is lowered through the water into the mercury well. The piece *c* is then put over *b*, and fixed in position on the rod *f*. A clean pipette, full of mercury, is then lowered into the trough and the point brought directly under the measuring tube, which is then lowered so that the pipette touches the top of the measuring tube. The bottle *p* is placed on the table, the screw clip *q* loosened and the air is forced over into the bulb *o* of the pipette. When the pipette tube has become filled with mercury, the measuring tube is raised and the pipette withdrawn. The air in the pipette is then displaced by mercury, and if the gas to be analyzed can be taken from a rubber tube, the end of this is slipped over the pipette tube and the gas then drawn in. The pipette is put into the well, placed under the measuring tube, which is lowered a little, and the gas forced into it by raising the bottle *p* to the position *p'* and opening the clip *q*. If desirable, the gas can be bubbled directly into the measuring tube by means of a properly bent tube. When as much gas as is desired is in the measuring tube, the pipette or other tube is withdrawn, the measuring tube raised or lowered until the top of the mercury in it is on a level with the top of the glass *u*, and if the mercury in *h* is not at the

same height it is adjusted by altering the height of the mercury in the trough, or by adding hot or cold water to that surrounding the tubes; care being taken to thoroughly mix the water by agitation with a stirrer. The gases in the measuring and pressure tubes are thus kept at the same pressure and temperature and are saturated with moisture. The reading of the measuring tube is effected by means of a telescope fixed at some distance from the apparatus. As the mercury is always at the same height, the telescope is always in the same position. When it is desired to subject the gas to the action of any reagent, a pipette containing a few drops of the reagent, the rest of the pipette being filled with mercury, is introduced into the trough and the gas drawn over as before described, and is then shaken up with the reagent. When the gas is to be transferred back to the measuring tube, mercury is drawn through the tube to remove any traces of the reagent, as it is essential that none of the reagent should be transferred with the gas. The point of the pipette being under the measuring tube, the bottle *p* is raised, the clip *q* opened, and the gas is forced into the measuring tube. When the gas is nearly over, the clip *q* is nearly closed, so that the gas passes quite slowly; the pipette is raised by a block of wood under its base, so that its point is 40 or 50 mm. above the surface of the mercury in the measuring tube. The progress of the liquid in the pipette tube must be watched carefully, and when it is about 20 mm. from the end, the clip *q* is closed, and by carefully pinching the rubber tube above the clip the liquid is forced within 1 or 2 mm. of the point of the pipette tube; the point of the pipette is then brought under the surface of the mercury, by raising the measuring tube, and the pipette withdrawn. The quantity of gas left in the pipette tube would be quite unmeasurable if it was five times larger.

It is convenient to have a pipette for every reagent. The pipette for explosions is manipulated in the same manner; the gases being exploded by an electrophorus. This explosion pipette, in many cases, enables the short measuring tube to take the place of a much longer one, and still be as accurate; since explosions can be made with just the requisite quantities of oxygen and the combustible gas, without any dilution. I have exploded illuminating gas and oxygen, and hydrogen and oxygen without any dilution, and I presume the bulb would resist much greater pressure, without fracture. I have found it easier to read the measuring tube, by having a piece of white paper pasted on the glass back of it and about 3 mm. above the top of the glass *u*. The pipettes used permit the complete transfer of the gases each way without the possibility of leakage, and they keep the reagents completely out of the measuring tube, where besides dirtying the surface of the mercury and thus rendering the reading uncertain, they might subsequently act injuriously

on the gas. Thus if any caustic potash was left in the measuring tube and any carbon compound exploded with oxygen, there would be found too little carbon dioxide, as part of it would be absorbed immediately after it was formed and before it could be measured. The only part of the apparatus that can be called delicate is the pipette tube, as it is rather long and comparatively unsupported. But with ordinary caution there is little danger of its breaking. If the apparatus had been constructed one-half longer than it is, its accuracy would have been increased and the system would have been nearly as manageable. This kind of apparatus is especially suited to short measuring tubes, for with long ones the pipette becomes fragile and unmanageable, besides being liable to have air drawn in through the rubber tube. The metal work is all of iron. Most of the work was done by myself and assistant. The only work that is difficult is in joining the tube to the bulb of the explosion pipette. The apparatus needs about twenty kilogrammes of mercury to work to advantage with four absorption pipettes.

The following analyses were made with this apparatus. They show the observations required and the manner of making the calculations.

Illuminating gas manufactured by the Boston Gas Light Co., and of an illuminating power of about eighteen candles. The two analyses here given were made with the same sample.

1st ANALYSIS.		2nd ANALYSIS.		
Divisions.	Volumes in C.C.	Divisions.	Volumes in C.C.	
147·3	22·90	149·7	23·26	Gas taken.
146·7	22·82	149·2	23·19	Carb. dioxide, absorbed by KOH.
146·5	22·79	149·0	23·16	Oxygen, absorbed by pyrogallate.
138·2	21·58	140·3	21·88	Hydrocarbons, " SO_3 in H_2SO_4.
326·4	48·91	328·0	49·14	Oxygen added.
84·9	13·72	82·8	13·41	Exploded.
8·8	2·49	5·9	2·06	Carbon dioxide absorbed.
64·0	10·65	45·3	7·88	Hydrogen added.
23·0	4·59	14·2	3·28	Exploded.

	1st.		Volumes.	Per cent.	
·47	N_2	(A) CH_4+CO+H_2 =21·11	22·90	100·00	Gas taken.
6·06	H_2O formed	(B) CH_4+CO =11.23	·08	·35	Carbon dioxide.
2·02	O_2 left	(C) $2CH_4+\frac{1}{2}CO+\frac{1}{2}H_2$ =25·31	·03	·13	Oxygen.
27·33	" added	$2C-A=3CH_4$ =29·51	1·21	5·29	Hydrocarbons.
25·31	" used	$A-B=H_2$ = 9·88	9·84	42·97	Methane (CH_4).
11·23	CO_2 formed	$B-CH_4=CO$ = 1·35	1·39	6·06	Carb. monoxide.
21·11	Combustible gas		9·88	43·15	Hydrogen.
			·47	2·05	Nitrogen.

	2nd.		Volumes.	Per cent.	
·53	N_2	(A) CH_4+CO+H_2 =21·35	23·26	100.00	Gas taken.
4·60	H_2O formed	(B) CH_4+CO =11·35	·07	·30	Carbon dioxide.
1·53	O_2 left	(C) $2CH_4+\frac{1}{2}CO+\frac{1}{2}H_2$ =25·73	·03	·13	Oxygen.
27·26	" added	$2C-A=3CH_3$ =30·11	1·28	5.50	Hydrocarbons.
25·73	" used	$A-B=H_2$ =10·00	10·04	43·16	Methane (CH_4).
11·35	CO_2 formed	$B-CH_4=CO$ = 1·31	1·31	5·63	Carb. monoxide.
21·35	Combustible gas		10·00	42·99	Hydrogen.
			·53	2·28	Nitrogen.

The hydrocarbons gave 17·70 per cent carbon dioxide by a subsequent determination. The calculations show that an error in the nitrogen determination affects the carbon monoxide, methane and hydrogen by the amount of the error. If in the place of the nitrogen found in the first analysis, we substitute that found in the second, we have CH_4 43·20 pr. ct.; for CO 5·85 per ct.; and for H_2 42·93 per ct.; and these numbers agree much better with the numbers of the second analysis. I have not been able to find any duplicate analyses of coal gas and not many of any kind, so I am not able to compare the accuracy of these analyses with that of analyses made with a different apparatus. Each of these analyses required from three to four hours for its execution.

Analyses of common Air.

Air collected April 25, 1874, freed from CO_2.

210·2	32·07	Air,	H_2O formed	20·15
317·2	47·57	H_2 added,	O_2	6·716
178·2	27·42	Exploded,	per cent of O_2	20·94

Same Sample.

197·7	30·25	Air,	H_2O formed	19·00
304·7	45·75	H_2 added,	O_2	6·333
173·6	26·75	Exploded,	per cent of O_2	20·93

Air collected April 26.

214·3	32·66	Air,	H_2O formed	20·52
311·9	46·81	H_2 added,	O_2	6·84
170·5	26·29	Exploded,	per cent of O_2	20·94

Same Sample.

207·7	31·70	Air,	H_2O formed	19·90
306·1	45·95	H_2 added,	O_2	6·633
168·8	26·05	Exploded,	per cent of O_2	20·92

Each of these air analyses required about half an hour for its execution. These analyses seem to have about the same degree of accuracy as the average of those made by Bunsen.

Gas generated by action of KOH on Fe and Zn.

	Div.	C.C.		
Air	193·5	29·64	18·47 H_2O formed	
Gas added	278·5	41·98	12·313 H_2 found	99·78 % H_2
Exploded	151·4	23.51	12·34 gas added	

Same Gas.

Air	166·3	25·68	14·07 H_2O formed	
Gas added	231·0	35·08	9·38 H_2 found	99·79 % H_2
Exploded	134·3	21·01	9·40 gas added	

These analyses show that the air was not quite expelled from the generating flask before the gas was collected.

This apparatus is especially useful where it is desired to obtain reliable results in a comparatively short time. As before mentioned, it was designed more especially for the analysis of illuminating gas. It will be noticed that the explosion pipette of this apparatus is nearly the same as the Cavendish eudiometer; the chief difference being in the manner of introducing and expelling the gas.

Office of Gas Inspection, Boston, May 14, 1874.

ART. XVIII.—*Researches on the Hexatomic compounds of Cobalt;* by WOLCOTT GIBBS, M.D.

[Continued from vol. vi, p. 116, August, 1873.]

5. THE salts described by Fremy* under the names of chloride, nitrate and sulphate of fuscocobalt contain also eight atoms of ammonia, and may be regarded as belonging to the octamin series. These salts have, according to Fremy, respectively the formulas:

$$Co_2(NH_3)_8.O.Cl_4+3OH_2$$
$$Co_2(NH_3)_8.O.(NO_3)_4+3OH_2$$
$$Co_2(NH_3)_8.O.(SO_4)_2+4OH_2$$

in modern notation. They are brown resinous masses, are difficult to obtain in a state of purity, and have as yet been but little studied. If we admit that the formulas are accurate, we may write them in accordance with the theoretic views which I have adopted, as follows:†

$$Co_2\left\{\begin{matrix}NH_3-Cl\\NH_3-Cl\\NH_3-NH_3\\NH_3-NH_3\\NH_3-Cl\\NH_3-Cl\end{matrix}\right\}>O\quad Co_2\left\{\begin{matrix}NH_3-NO_3\\NH_3-NO_3\\NH_3-NH_3\\NH_3-NH_3\\NH_3-NO_3\\NH_3-NO_3\end{matrix}\right\}>O\quad Co\left\{\begin{matrix}NH_3\\NH_3\\NH_3-NH_3\\NH_3-NH_3\\NH_3\\NH_3\end{matrix}\right\}>SO_4\ \Big\}>O\ \Big\}>SO_4$$

Jörgensen‡ suggests that these salts may contain hydroxyl in place of oxygen. There is at present no method of deciding the question, and I have adopted the view which seems to me the most probable. Künzel's hyposulphate above mentioned may be regarded as belonging to this series, and as having the structural formula:

$$Co_2\left\{\begin{matrix}NH_3\\NH_3\\NH_3-NH_3\\NH_3-NH_3\\NH_3\\NH_3\end{matrix}\right\}>S_2O_6\ \Big\}>O\ \Big\}>S_2O_6$$

* Ann. de Chimie et de Physique, [3,] tome xxxv, p. 257.

† Blomstrand has given the same formulas with trifling variations. Chemie der Jetztzeit, p. 355.

‡ Gmelin-Krauts' Handbuch, vol. iii, p. 468.

but, according to Geuther, the formula given by Künzel must be tripled, and the salt then belongs to the dodekamin or luteocobalt series. In the absence of direct proof of the existence of luteocobalt in this salt, Künzel's formula appears the more probable of the two. The compounds above mentioned, with those which I have myself described, form the only known members of the octamin group, a further study of which will doubtless yield an ample return.

6. *Action of ammonic nitrite on salts of cobalt.*—To obtain a clear view of the nature and mode of formation of the salts of xanthocobalt, I have carefully studied the relations of ammonic nitrite to salts of cobalt under different conditions. This subject has already been examined by Erdmann, and in my laboratory by Sadtler. Erdmann found that when a neutral solution of cobaltic chloride is mixed with a neutral solution of ammonic nitrite no turbidity ensues, but after spontaneous evaporation in the air a salt crystallizes, with the formula, as Erdmann writes it (old style):

$$Co_2\overset{\frown}{O_3}.2NH_3,\ 3NO_3+NH_4O,\ NO_3.$$

This salt is isomorphous with the corresponding potassium salt, the crystals belonging to the rhombic system. Erdmann does not explain the reaction which takes place in the formation of this or the corresponding potassium salt, and regards the compounds in question as double salts. When slightly acid solutions were employed, Erdmann obtained, in addition to the above mentioned salt, an ammonic salt corresponding to Fischer's salt, $Co_2(NO_2)_{12}(NH_4)_6+3OH_2$, as we should now write it. The existence of this salt was first remarked by Genth and myself.* Sadtler studied the action of ammonic nitrite on acid solutions of cobaltic chloride, and obtained two salts having respectively the formulas:

$$Co_2(NO_2)_{10}(NH_4)_4+2OH_2$$
$$Co_2(NO_2)_{12}(NH_4)_6+2OH_2,$$

but did not observe the formation of Erdmann's ammonium salt. In repeating these experiments I always obtained Erdmann's ammonium salt, $Co_2(NH_3)_4(NO_2)_8(NH_4)_2$, in largest quantity. The crystals are uncommonly beautiful and well defined. Of these crystals

0·3390 gr. gave 0·1783 gr. SO_4Co=20·02 per cent.

The formula requires 20·00 per cent. In one experiment, in which a little free acetic acid was present, I obtained large dark sherry-wine colored prismatic crystals, which after solution and recrystallization gave only very thin lozenge-shaped tabular

* This Journal, 2d Series, vol. xxiv, p. 86.

crystals, the form and appearance of which are highly characteristic. These crystals gave no reactions with salts of luteocobalt, purpureocobalt and roseocobalt, and none with potassic chromate and dichromate, ammonic oxalate or argentic nitrate. The absence of the first mentioned reactions shows that they do not contain $Co_2(NH_3)_4(NO_2)_8$ or $Co_2(NO_2)_{12}$, while the fact that they give no reactions with alkaline chromates and oxalates shows that they do not contain any known cobaltamin. Of these crystals

0·1554 gr. gave 0·0974 gr. SO_4Co=23·86 per cent cobalt.
0·3081 gr. gave 0·0635 gr. NH_3 =20·61 per cent ammonia.

The formula $Co_2(NH_3)_6(NO_2)_6$ requires

Cobalt,	23·79	23·86
Ammonia,	20·56	20·61

These analyses are sufficient to identify the salt in question with one which Erdmann has described in the paper referred to, as formed by the action of ammonia and potassic nitrite upon cobaltic chloride, unfortunately with but very scanty details. I attribute to this salt the formula

$$Co_2\begin{cases} NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \\ NH_3-NO_2 \end{cases}$$

and consider it to be the nitrous representative of the hexamin $Co_2(NH_3)_6$. I have not succeeded in obtaining from it other members of the same series; but it is, to say the least, probable that the dichrocobalt-chloride of Fr. Rose,* $Co_2(NH_3)_6Cl_6)+2OH_2$, represents the corresponding chloride. Künzel† has described a sulphite to which he attributes the formula

$$Co_2(NH_3)_6(SO_3)_3+OH_2,$$

but according to Geuther‡ this formula must be doubled, the salt belonging to the dodekamin or luteocobalt series, with the formula

$$Co_2(NH_3)_{12}(SO_3)_6+Co_2(SO_3)_6+2OH_2.$$

Erdmann's hexamin salt is of special interest because, as I shall show, it forms the first term in a remarkable series of metameric bodies; its formation under the circumstances may with great probability be expressed by the equation:

$$2CoCl_2+10NH_4.NO_2+3O=Co_2(NH_3)_6(NO_2)_6+4NH_4Cl+3OH_2,$$

* Untersuchungen über ammoniakalische Kobalt-Verbindungen. Heidelberg, 1871.

† Journal für prakt. Chemie, 72, p. 209. ‡ Ann. de Pharmacie, 128, p. 127.

as the salt is not formed immediately, but only after absorption of oxygen from the air. The formation of Erdmann's ammonium salt may in like manner be represented by the equation:

$$2CoCl_2+8NH_4.NO_2+2O=Co_2(NH_3)_4(NO_2)_6+4NH_4Cl+2OH_2,$$

the presence of oxygen being necessary in this case also.

In another experiment I obtained no hexamin nitrite, but only Erdmann's ammonium salt and the two salts described by Sadtler, and to which he gave respectively the formulas:

$$Co_2(NO_2)_{10}(NH_4)_4+2OH_2$$
$$Co_2(NO_2)_{12}(NH_4)_6+2OH_2.$$

These last salts were found in considerable quantity mixed together as a yellow sparingly soluble crystalline powder, when a strong solution of ammonic nitrite was poured upon finely pulverized cobaltic chloride, and acetic acid was added in small excess. I consider the formation of these two salts to be represented by the equations:

$$2CoCl_2+10NH_4.NO_2+3O=Co_2(NO_2)_{10}(NH_4)_4+6NH_3+3OH_2$$
$$2CoCl_2+12NH_4.NO_2+3O=Co_2(NO_2)_{12}(NH_4)_6+8NH_3+3OH_2.$$

Professor Sadtler has shown that in these cases also an absorption of oxygen from the air takes place. When a solution of ammonic nitrite is added to a strong alcoholic solution of cobaltic chloride, Erdmann's ammonium salt, $Co_2(NH_3)_4(NO_2)_6(NH_4)_2$, is chiefly formed, and only a small quantity of the four and six-atom salts. The compound formed crystallizes from the alcoholic solution in very beautiful and well defined prismatic forms.

From the above it will be seen that at least four distinct compounds are formed by the action of ammonic nitrite upon solutions of cobaltic chloride in presence of a weak acid and of the oxygen of the air. It is at least probable that all four are formed at the same time, though in varying proportions. I have already shown that, in the presence of free ammonia and of ammonic nitrate, cobaltic chloride and ammonic nitrite yield the nitrate of the octamin series. Of the action of ammonic nitrite upon cobaltic salts in the presence of free ammonia, I shall speak in treating of the formation of the salts of xanthocobalt.

7. I have stated above that Erdmann obtained the hexamin nitrite, $Co_2(NH_3)_6(NO_2)_6$, by the joint action of potassic nitrite and ammonia upon cobaltic chloride. On repeating his experiments, I found that small quantities of this salt were formed, but that the chief products of the action were salts of xanthocobalt, the formation of which Erdmann does not appear to have noticed. Small quantities of salts of the octamin series are also formed. The filtered solution obtained in this reaction

was precipitated by potassic dichromate, and the orange-red needles obtained recrystallized for analysis; of these crystals

0·6145 gr. gave 0·7393 gr. CrO_4Ba=51·40 per cent Cr_2O_7.
0·7712 gr. gave 0·9277 gr. CrO_4Ba=51·40 per cent Cr_2O_7.
0·5615 gr. gave 96·5 c.c. nitrogen (moist) at 15° C. and 763^{mm}·1= 20·12 per cent nitrogen.
0·5028 gr. gave 86 c.c. nitrogen (moist) at 15° C. and 763^{mm}·1= 20·05 per cent nitrogen.

The formula $Co_2(NH_3)_{10}(NO_2)_2Cr_2O_7$ requires 53·22 per cent Cr_2O_7 and 20·67 per cent nitrogen, while the formula of the octamin salt, $Co_2(NH_3)_8(NO_2)_4Cr_2O_7$, requires 32·91 per cent Cr_2O_7 and 19·30 per cent nitrogen, so that the analyses leave no reasonable doubt that the salt was a mixture of a salt of xanthocobalt with a smaller proportion of the corresponding salt of the octamin series.

The above results clearly show that the action of ammonic nitrite upon salts of cobalt in presence of free acid is extremely complex, not less than six classes of salts being formed, of which two belong certainly to basic series, while three may be regarded as salts of ammonium. The sixth, $Co_2(NH_3)_6(NO_2)_6$, is probably also one term in a hexamin series.

8. The ammonia-nitrites discovered by Erdmann are of especial interest. They present the first and at present the only known instance in which cobalt, by uniting with ammonia and nitroxyl, NO_2, forms an electro-negative or chlorous radical. The compound $Co_2(NH_3)_4(NO_2)_8$ may be regarded as existing in combination with two atoms of a mon-atomic radical, exactly as the compound $Co_2(NH_3)_8(NO_2)_4$ combines with two atoms of chlorine. The structural formulas may be written respectively:

$$Co_2\left\{\begin{array}{l}NH_3-NO_2\\NH_3-NO_2\\NH_3-NH_3-Cl\\NH_3-NH_3-Cl\\NH_3-NO_2\\NH_3-NO_2\end{array}\right. \qquad Co_2\left\{\begin{array}{l}NH_3-NO_2\\NH_3-NO_2\\N\!<\!\begin{matrix}O\\O\end{matrix}\!>\!N-O-OK\\N\!<\!\begin{matrix}O\\O\end{matrix}\!>\!N-O-OK\\NH_3-NO_2\\NH_3-NO_2\end{array}\right.$$

With these formulas we may advantageously compare those of chloride of luteocobalt, of Fischer's salt considered as anhydrous, and of chloride of xanthocobalt:

$$Co_2\begin{cases} NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \end{cases} \qquad Co_2\begin{cases} N<\overset{O}{\underset{O}{>}}N-O-OK \\ N<\overset{O}{\underset{O}{>}}N-O-OK \\ N<\overset{O}{\underset{O}{>}}N-O-OK \\ N<\overset{O}{\underset{O}{>}}N-O-OK \\ N<\overset{O}{\underset{O}{>}}N-O-OK \\ N<\overset{O}{\underset{O}{>}}N-O-OK \end{cases}$$

$$Co_2\begin{cases} NH_3-NO_2 \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NH_3-Cl \\ NH_3-NO_2 \end{cases}$$

The manner in which these compounds may be derived from each other by replacement is sufficiently obvious, and is best seen by assuming chloride of luteocobalt and Fischer's salt as the two extreme terms of the series in which the other three are intermediate.

Erdmann's analyses leave no reasonable doubt as to the constitution of the ammonia-nitrites. I have thought it worth while, however, to make a few additional analyses in support of his view. In the potassium salt:

0·4497 gr. gave 0·3397 gr. SO_4Co and SO_4K_2=75·54 per cent.
0·7338 gr. gave 0·5615 gr. " " =76·52 per cent.
0·5937 gr. gave 127 c.c. nitrogen at 6°·5 C. and $773^{mm}\cdot4$=26·45 per cent nitrogen.

The formula $Co_2(NH_3)_4(NO_2)_8K_2$ requires 76·58 per cent $2SO_4Co+SO_4K_2$ and 26·58 per cent nitrogen. In the silver salt:

0·3580 gr. gave 0·2902 gr. SO_4Co and SO_4Ag_2.
0·5937 gr. gave 0·1675 gr. silver=28·21 per cent.

The cobalt by difference amounts to 15·33 per cent. The formula $Co_2(NH_3)_4(NO_2)_8Ag_2$ requires 28·05 per cent silver and 15·32 per cent cobalt.

Thallium salt.—When a solution of the potassium salt is added to one of thallous nitrate, a beautiful sherry-wine-colored crystalline precipitate is thrown down, which on recrystallization gives very well defined prismatic crystals, having apparently the same form as the corresponding potassium and ammonium salts.

Mercurous salt.—A solution of potassic ammonia-cobalt-nitrite gives immediately in solutions of mercurous nitrate a beautiful

orange-colored crystalline precipitate, which may be dissolved in boiling water, but not without partial decomposition. The salt does not crystallize well from the solution. Of this salt:

0·7785 gr. gave 0·1775 gr. SO_4Co=8·68 per cent cobalt.

The formula $Co_2(NH_3)_4(No)_2(Hg_2)_2$ requires 8·71 per cent.

A solution of the potassic salt gives no precipitate with salts of cobalt, nickel, barium and copper, and none at first with plumbic acetate. After standing, however, a lead salt separates in fine acicular leafy crystals of a brown-orange color, soluble in hot water, but with partial decomposition. The same is true of the silver salt, but small quantities of this may usually be dissolved and recrystallized without change. The silver salt is extremely well characterized; its moderate degree of solubility and the facility with which it crystallizes in tabular lustrous crystals have made it of great service in my investigations, especially in distinguishing salts containing $Co_2(NH_3)_4(NO_2)_8$ from those which contain $Co_2(NO_2)_{12}$. Compounds of ammonia-cobalt-nitrite with barium, strontium, etc., are easily formed by double decomposition, the metallic chlorides being digested with a solution of the argentic salt. They are pale orange-yellow, soluble salts, which I have not further examined. A solution of the potassic salt gives beautiful crystalline precipitates with salts of various organic alkaloids, especially with those of brucin and strychnin. These are soluble in hot water without sensible decomposition, and may be recrystallized. Salts of anilin give a bright yellow precipitate with potassic ammonia-cobalt-nitrite, which is, however, immediately decomposed, phenol being set free. The potassic salt gives also splendid crystalline precipitates with salts of croceocobalt, xanthocobalt, luteocobalt, etc. I have already noticed the salt of croceocobalt, and will describe the salts of the other bases in due course.

Erdmann has not attempted to explain the formation of this class of salts. He remarks that a yellow insoluble compound is formed at the same time with the potassic salt $Co_2(NH_3)_4(NO_2)_8K_2$, which appears to be a mixture which cannot be obtained pure for analysis. I have also obtained this body, and also regard it as consisting mainly of Fischer's salt, $Co_2(NO_2)_{12}K_6$, though as Erdmann states, it contains a small percentage of ammonia. The formation of the salt $Co_2(NH_3)_4(NO_2)_8K_2$ may be expressed by the equation

$$2CoCl_2+4NH_4Cl+8KNO_2+O=Co_2(NH_3)_4(NO_2)_8K_2+6KCl+2HCl+OH_2,$$

if we suppose oxygen to be absorbed from the air. In consequence, however, of the formation of free chlorhydric acid, N_2O_3 is set free, and it is much more probable that this is reduced by the nascent-hydrogen; so that we have

$$N_2O_3+2H=2NO+OH_2.$$

The potassium salt is also formed, as I shall show, in various other cases; the similarity of some of its reactions to those of a solution of $Co_2(NO_2)_{12}Na_6$ in sodic nitrite for a long time misled me; but its relations to salts of silver, mercury and thallium enable us to recognize its presence with absolute certainty. The salt does not enter into combination with iodine.

XANTHOCOBALT.

9. Genth and I have shown in our memoir that the salts of xanthocobalt may be formed either directly by the action of nitrous acid vapors upon salts of cobalt, or by the action of the same acid upon salts of purpureocobalt and roseocobalt, in each case in the presence of free ammonia. I propose now to give the results of a more detailed study of the subject.

With respect to the constitution of this class of salts, I may remark, in the first place, that Genth and I left it undecided whether the salts in question contain NO or NO_2, pointing out the fact that the analyses do not decide in favor of either view, and adopting the former provisionally. Braun first proved conclusively that the salts of xanthocobalt contain NO_2, and this view has since been generally adopted. I have already shown (§ 1) that when cobaltic chloride, $CoCl_2$, is mixed with ammonia and ammonic nitrite and nitrate, the solution absorbs oxygen from the air, while the nitrate of the octamin series, $Co_2(NH_3)_8(NO_2)_4(NO_3)_2$, is formed. I have not observed in this reaction the formation of a salt of xanthocobalt. If present at all, such salts must be in very small relative quantity. Genth and I have shown, on the other hand, that when the red gases resulting from the action of nitric acid upon starch, sawdust or arsenous oxide are passed into solutions of cobaltic salts in presence of an excess of ammonia, salts of xanthocobalt are formed in a very short time, and in large quantity.

If we consider the red gas to consist of hyponitric oxide, N_2O_4, we may have

$$2Co(NO_3)_2+10NH_3+N_2O_4=Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4.$$

In preparing sulphate and nitrate of xanthocobalt by this process, I have on several occasions been able to detect only salts of this base among the products of the reaction. In one case, however, in which I employed cobaltic sulphate and added so large a quantity of ammonic sulphate that the solution gave no precipitate with ammonia, I obtained a very large relative quantity of Erdmann's salt $Co_2(NH_3)_6(NO_2)_6$. In other cases in which cobaltic chloride was present I detected crystals of the chloronitrate $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2$. The solutions after the action of the red gases also contain small quantities of the ammonia-cobalt-nitrite of ammonium, $Co_2(NH_3)_4(NO_2)_8(NH_4)_2$, as well as ammonic nitrite and nitrate.

On the other hand, however, I have already shown (§ 3) that salts of this radical are formed in large quantity, together with a smaller proportion of the octamin nitrate, by the action of a mixture of potassic nitrite and ammonia upon cobaltic nitrate in presence of air; but that xanthocobalt is exclusively formed by the action of the same mixture upon a solution of ammonic and cobaltic sulphates. I am unable to offer any plausible explanation for the difference of the products in the two cases.

When cobaltic nitrate, ammonic nitrite and ammonia are mixed and placed in a tightly-corked bottle, no action whatever appears to take place, even after the mixture has stood some days. But if plumbic hyperoxide, PbO_2, is added, the mixture soon becomes yellow, and after a few hours large crystals of nitrate of xanthocobalt are formed with distinct reduction of the plumbic hyperoxide. The reaction in this case may be represented by the equation:

$$2Co(NO_3)_2+10NH_3+2NO_2.NH_4+PbO_2=Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4+PbO+(NH_4)_2O.$$

Potassic hypermanganate may also be employed as an oxidizing agent, but is less convenient. The experiment just detailed appears to me to render it most probable that in the action of the red gases upon salts of cobalt in presence of ammonia, the resulting salts of xanthocobalt are not formed by the direct union of the cobaltic salt with ammonia and nitroxyl, but that ammonic nitrite is first formed, and that the oxygen necessary for the completion of the reaction is derived from the decomposition of some element of the complex mixture of NO, NO_2, N_2O_3 and NO_3H, which make up the red vapors.

The formation of salts of xanthocobalt by the action of the red gas upon salts of purpureocobalt and roseocobalt in the presence of free ammonia is easily explained. We have here simple cases of double decomposition, a particular instance of which, covering in substance the whole ground, may be expressed by the equation:

$$Co_2(NH_3)_{10}(NO_3)_6+2NH_4.NO_2=Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4+2NO_3.NH_4.$$

Salts of xanthocobalt are always formed when salts of purpureocobalt and roseocobalt are heated or even digested in the cold with alkaline nitrites. I have made a special study of the action of potassic and sodic nitrites upon chloride of purpureocobalt, the details of which are as follows:

10. *Action of sodic and potassic nitrites upon chloride of purpureocobalt.*—A quantity of chloride of purpureocobalt was dissolved in boiling water, with a little free acetic acid to prevent decomposition, and added to a hot solution of potassic nitrite in excess. The dark brown-red solution was evaporated at a

gentle heat to half its volume. On cooling, a small quantity of Fischer's salt $Co_2(NO_2)_{12}K_6+2OH_2$ separated; afterward sherry-wine colored prismatic crystals were formed in abundance. After recrystallization these were analyzed.

0·2824 gr. gave 0·1519 gr. $CoSO_4$=20·47 per cent cobalt.
0·5557 gr. gave 0·2092 gr. silver=12·37 per cent chlorine.

The same experiment was made with sodic nitrite, and with similar results. After two recrystallizations the salt formed was analyzed.

0·4163 gr. gave 0·2235 gr. $CoSO_4$=20·43 per cent cobalt.
0·2332 gr. gave 0·0876 gr. silver=12·38 per cent chlorine.
0·6625 gr. gave 192·12 c.c. nitrogen (moist) at 14° C. and 764^{mm}·1 =34·29 per cent.
1·2310 gr. gave 0·5825 gr. water=5·24 per cent hydrogen.
1·6542 gr. gave 0·7996 gr. water=5·37 per cent hydrogen.

The salt being found anhydrous, the analyses agree with the formula:

$$Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2,$$

which requires

		Found.	
Cobalt	20·52	20·47	20·43
Chlorine	12·34	12·37	12·38
Hydrogen	5·26	5·24	5·37
Nitrogen	34·09	34·29	

and which is fully sustained by other considerations, as I shall show. As the solutions of the alkaline nitrites employed also contained nitrates, the formation of the new salt may be represented by the equation:

$$Co_2(NH_3)_{10}Cl_6+2KNO_2+2KNO_3=4KCl+Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2.$$

The salt itself is then a nitroso-chloro-nitrate, and belongs probably to the α-dekamin or purpureocobalt series; but it may be more conveniently regarded as the chloro-nitrate of xanthocobalt. It has the wine color of the salts of the so-called xanthocobalt series, but crystallizes usually in prismatic forms, which are moderately soluble in hot water, and separate readily from the solution. With neutral potassic chromate the salt gives the beautiful yellow crystalline chromate of xanthocobalt:

$$Co_2(NH_3)_{10}(NO_2)_2(CrO_4)_2+2OH_2.$$

With potassic ferrocyanide it gives the characteristic red prismatic crystals of

$$Co_2(NH_3)_{10}(NO_2)_2FeCy_6+6OH_2,$$

and with ammonic oxalate, oxalate of xanthocobalt,

$$Co_2(NH_3)_{10}(NO_2)_2(C_2O_4)_2,$$

the reactions being too obvious to require explanation by equations.

As it is difficult to prevent the action of the alkaline nitrites upon chloride of purpureocobalt from going too far and decomposing the new salt first formed, I had recourse to a different mode of preparation, by which the salt can be prepared in any quantity and with the greatest facility. A hot solution containing one molecule of chloride of xanthocobalt was mixed with a solution containing one molecule of nitrate of xanthocobalt. On cooling, the nitroso-nitro-chloride crystallized in beautiful prismatic forms. In this case we have

$$Co_2(NH_3)_{10}(NO_2)_2Cl_4+Co_2(NH_3)_{10}(NO_2)_2(NO_3)_4=2Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2.$$

Of the crystals so formed

0·6203 gr. gave 0·3310 gr. $CoSO_4$=20·31 per cent cobalt.
0·9268 gr. gave 0·3450 gr. silver=12·24 per cent chlorine.

The formula requires 20·51 per cent cobalt and 12·34 per cent chlorine. A portion of the crystallized salt was dissolved and precipitated by argentic nitrate. The filtrate from AgCl gave on evaporation crystals of nitrate of xanthocobalt, in which

0·2972 gr. gave 0·1469 gr. $CoSO_4$=18·81 per cent cobalt.

The formula of the nitrate requires 18·73 per cent. These results leave no doubt as to the constitution and true relations of the chloro-nitrate.

Gold salt.—When the chloro-nitrate is dissolved and a solution of aurochloride of sodium, $AuCl_4Na$, is added in excess, long prismatic wine-yellow crystals are formed. Of these crystals

0·8564 gr. decomposed by zinc and sulphuric acid gave 0·6300 gr. silver=24·16 per cent chlorine and 0·2858 gr. gold=33·36 per cent.

0·4084 gr. gave 0·1770 gr. Au+Co=43·34 per cent and by difference 9·98 per cent cobalt.

This formula, $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2+2AuCl_3$, requires

		Found.
Cobalt	9·98	9·98
Gold	33·33	33.36
Chlorine	24·03	24·16

The salt is readily decomposed by boiling with reduction of metallic gold.

Platinum salt.—Platinic chloride in solution precipitates the chloro-nitrate almost immediately in the form of wine-yellow needles. After recrystallization this salt was analyzed with the following results:

0·6405 gr. fused with potassio-sodic carbonate gave 0·5564 gr. silver=28·55 per cent chlorine, 0·1986 gr. platinum=31·00 per cent, and 0·0597 gr. cobalt=9·33 per cent.

The platinum and cobalt were weighed together as metals after reduction by hydrogen, and the cobalt was then dissolved by long boiling with nitric acid.

This formula, $Co_2(NH_3)_{10}(NO_2)_2(NO_3)_2Cl_2 + 2PtCl_4$, requires

		Found.
Cobalt	9·40	9·33
Platinum	31·55	31·00
Chlorine	28·28	28·55

The salt had no water on heating to 140° C.

(To be continued.)

ART. XIX.—*On the Mechanism of Stromboli;* by ROBERT MALLET, M.A., F.R.S.

[Abstract of a paper received by the Royal Society of London, May 17, 1874, and read June 25, 1874.]

STROMBOLI stands unique (omitting the as yet imperfectly known Masaya in Central America), amongst the volcanoes of our globe as characterized by the rhythmical recurrence of its outbursts, which have continued with but little alteration for more than 2000 years. The phenomena of Stromboli have been more or less accurately described by several authors, from Spallanzani and Hoffman to Scrope and Daubenay. The last but one of these has proposed an explanation of the phenomena presented by the recurrent outbursts at short intervals of time, which within rather narrow limits are constant, and in the traditional convictions are supposed to have some connection with the state of the weather or of the barometer. This author points out that these explanations fail wholly to account for the long continued rhythmical action of this volcano, and for any presumable connection with the weather.

The writer, in company with Colonel Henry Yule, B.E. (well-known for his embassy in Siam, as editor of Marco Polo's Travels, and for other important labors), examined Stromboli and other islands of the Lipari group in 1864, and describes more systematically and minutely than has been done by previous writers, to his knowledge, the circumstances and phenomena of rhythmical eruption presented by the volcano. He then briefly refers to the history of observation as respects the geysers of Iceland; to some of the circumstances, as to those experimentally ascertained by Henderson, and to the

able explanation of their rhythmical action given by Bunsen and Des Cloizeaux. He then points out that Stromboli presents the rhythmic recurrence of outburst of a geyser combined with the eruptive action of a volcano of small energy, and suggests, aided by a diagram, the very simple mechanism by which this combination is produced at Stromboli; and, comparing the action of such mechanism with the actual phenomena presented by the volcano, shows that they tally with each other minutely. The crater of Stromboli consists of a deep cavity, at bottom of which is a tundish-shaped funnel, formed chiefly of the loose material which after each outburst falls back into the crater. More or less liquid lava finds its way by a lateral duct into the bottom of this funnel of loose material, which it partially solders together in the period between successive outbursts. Below this mass of loose material, and penetrating to a depth considerably below the sea level, which is about 400 feet below the bottom of the crater, is the tube which in all ordinary volcanoes carries up together the mixed steam and liquid lava that are erupted through it and the crater. In this instance the tube is supplied with water percolated from the sea, which is heated by the high temperature of the ducts through which it passes by the heated walls of the tube, and by jets of superheated steam driven into its lower extremity by ducts, which, like those that conduct the lava to the bottom of the crater above, are derived from the great volcanic channels much more centrally situated in the island, and belong to the great craters by which the island itself was formed. The tube with its water and steam ducts thus performs the rhythmical functions of alternate outbursts and quiescence that belong to the geyser, and the ascending column of water in the tube, when the boiling point, due to statical depth, superincumbent tension, and obstructions at the bottom of the crater, has been reached for all parts of its height, is driven forth, throwing the loose material mingled with liquid lava out of the crater before it in a cloud of stones, dust, shreds of semi-liquid lava, and steam, and perhaps pulverized water. The main mass of the solid ejecta fall back into the crater, to be again expelled by the next outburst.

Several facts and details are adduced tending to corroborate this as being the true mechanism of this singular volcanic vent, and several inaccuracies of description of preceding writers, more especially the statement that the bottom of the crater is about 2000 feet above the level of the sea, are corrected—the author's levels being derived from approximate barometric measurements. The author then refers to the old traditions still current as to connections in the way of cause and effect between the phenomena of the volcano and those of weather, etc. He points out that the explanation previously given of

the supposed mechanism of Stromboli fail as completely to account for any such connection as they do to explain the rhythmical action of the volcano itself. The only distinct relations that can be gathered from the inhabitants of the Lipari group, between the appearances presented by Stromboli with different states of weather, wind or atmospheric pressure, are shown to be not those of direct cause and effect, but to be referable to acknowledged meteorological principles.

ART. XX.—*Brief Contributions from the Physical Laboratory of Harvard College.* No. XI.—*Increase of Magnetism in a bar of soft iron upon the reversal of the magnetizing current;* by WILLIAM A. BURNHAM.

IN testing the intensity of the magnetism induced in a bar of soft iron which forms the armature common to two electro-magnets, according to the method described by Mr. Sears in the July number of this Journal for the present year, the galvanometer indicates an increase in intensity of the magnetic state of the bar on the first passage of the current through the magnetizing helices, after a reversal of the poles of the battery. This increase disappears upon subsequent magnetizations of the bar. Various observations were taken by placing the induction coil C (see paper by Mr. Sears in the July number of this Journal) at different distances on the bar which was magnetized. A bar of soft iron was experimented with in the first instance, the coil C being at a distance of five centimeters from the middle or zero point. A Thomson's reflecting galvanometer was used and the readings were taken when the circuit was made and also when it was broken. The following table shows the results with C at various distances upon the bar.

TABLE I.

Distances of C from the middle of the bar in centimeters.	5		6		7		8		9		10	
Constant deflections on making and breaking circuit.	+ 40	− 40	+ 50	− 50	+ 60	− 60	+ 70	− 70	+ 80	− 80	+ 90	− 90
Deflection on the first passage of the current after changing the poles.	− 90	+ 50	− 110	+ 60	− 130	+ 70	− 150	+ 80	− 170	+ 90	− 190	+ 100
Return to constant deflections on second and succeeding passages of the current.	− 40	+ 40	− 50	+ 50	− 60	+ 60	− 70	+ 70	− 80	+ 80	− 90	+ 90

It will be seen by this table, that when the coil C stands at a distance of five centimeters from the zero point on the armature upon making the circuit, the galvanometer needle swings

to the left 40°, and upon breaking the circuit it is deflected 40° to the right. The signs in our table denote this change of direction. When the poles are reversed and the circuit made, it swings 90° to the right, and when broken 50° to the left, returning to the constant deflection of 40° upon the second and each succeeding passage of the magnetizing current while the poles remain the same. In the same manner, when the coil stands at 6, 7, 8, etc., the first swings of the galvanometer needle are respectively 50 to 50, 60 to 60, etc., increasing in regular proportion upon each reversal of the poles and returning upon each second and succeeding passages of the current to constant deflections. The following are the results when a bar of steel was substituted for the soft iron.

TABLE II.

Distances of C from the middle of the bar in centimeters.	5		6		7		8		9		10	
Constant deflections on making and breaking circuit.	+ 20	− 20	+ 30	− 30	+ 40	− 40	+ 50	− 50	+ 60	− 60	+ 70	− 70
Deflections on the first passage of the current after changing the poles.	− 40	+ 25	− 50	+ 35	− 60	+ 45	− 70	+ 55	− 80	+ 65	− 90	+ 75
Return to constant deflections on second and succeeding passages of the current.	− 20	+ 20	− 30	+ 30	− 40	+ 40	− 50	+ 50	− 60	+ 60	− 70	+ 70

In the case of steel, when the coil C stands at a distance of five centimeters from zero, the galvanometer needle swings from +20° to −20°. Upon reversing the poles it is deflected +40° and −25°, and returns to 20° upon the second passage of the magnetizing current. By comparing Tables I. and II. it will be seen that the increase of magnetization after a reversal of the poles is not so great in iron as in steel, owing doubtless to permanent magnetism in the steel.

No. XII.—*On the Effect of Longitudinal Vibrations upon Electro-magnets;* by E. L. CARNEY.

IF a bar of iron or steel which is rendered magnetic by a magnetizing spiral receives a sharp blow or shock, an induced current is generated in a coil of fine wire, which is slipped upon the bar and is connected with a galvanometer. This current is opposite in direction to that of the magnetizing current. (Wiedemann, *Galvanismus und Elektromagnetismus*, 1863, pp. 375, 397. Also Dr. Emil Villari, *Ueber den transversalen Magnetismus des Eisens und des Stahles*, Pogg. Ann., cxxxvii, 1869, pp. 569–591.) This investigation was undertaken to ascertain the

effect of the excursion to and fro of the particles of the iron in the state of longitudinal vibration. A fine coil, which we shall term C, was slipped upon the bars which were experimented upon and connected with a reflecting galvanometer. The rods —130 cm. in length and 1 cm. in diameter—were firmly clamped at their middle points. In the experiments which are comprehended in Tables I. and II. the magnetizing coil was placed at right angles to the rod or bar. In the experiments given in the Tables III. and IV. the rod formed the core of the magnetizing helix. We shall term the currents which were excited in the fine wire coil C, which was connected with the galvanometer, on the making of the magnetizing circuit, secondary currents; and the currents which were excited in the same coil by longitudinal vibrations, tertiary currents.

Table I.—The magnetizing circuit was made and broken instantly. On exciting longitudinal vibrations, it was found that a tertiary current resulted which was opposite in direction to the secondary current due to making the magnetizing circuit, and corresponded in direction to that of the secondary current resulting from breaking the magnetizing or primary circuit. With steel the results obtained were constant, while with iron they were variable. It was found impossible to excite more than one tertiary current.

I have called the current made by depressing the key on the magnetizing circuit positive throughout these experiments.

On the iron rod the magnetizing helix was forty centimeters from the center; on the steel rod only fifteen.

TABLE I.

Iron rod.		*Steel rod.*	
Current made.	Current caused by vibrations.	Current made.	Current caused by vibrations.
+	−	+	−
55	35	30	10
50	45	30	10
70	45	30	10
55	44		
53	40		
60	38		

Table II.—In this case the key was depressed and kept so, while both secondary and tertiary currents were obtained. In some cases there was more than one tertiary current, which was apparently due to the feebleness of the first vibration, as the number of deflections so obtained was more noticeable when such was the case. However, this could not have been the sole cause, for more than one tertiary current appeared when the vibration was all that could have been wished for. In the

case of the iron rod it will be noticed that the tertiary vibrations are greater than the secondary, while the reverse is true with the steel rod.

The current was then broken and the amount of deflection noticed; and afterward a tertiary current obtained in the usual way. The latter currents were opposite in direction to those previously described. As there is but one instance of more than one tertiary being obtained, it was probably due to the feebleness of the first vibration.

In all these experiments the tertiary currents were in the *same direction* as the secondary ones, differing in this respect from those of Table I.

The magnetizing helix in both cases was fifteen centimeters from the center of the rods.

Iron rod.

Current made.	Current caused by vibrations.	Current broken.	Current caused by vibrations.
+	+	−	−
40	60–10–10	45	40–5
47	50–5–10–5	50	45
42	40–15	40	30
15	20	10	30
15	30	17	30
17	32	16	32

Steel rod.

Current made.	Current caused by vibrations.	Current broken.	Current caused by vibrations.
+	+	−	−
20	10	25	10
22	7	27	15
23	5–3	29	15
20	10	20	10

Table III.—Here the magnetizing coil was placed on the rods instead of at right angles to them, as in the previous cases, otherwise the experiment was performed as described in Table II.

The only noticeable difference was the increased strength of the deflections, and the entire absence of more than one tertiary current. In the case of the iron rod, when the current *was broken* the tertiary current was greater than the secondary, while in Table II. the tertiary current was greater when the current *was made.*

On the iron rod the magnetizing helix was distant forty centimeters from the center; on the steel rod twenty-two.

Iron rod.

Current made.	Current caused by vibrations.	Current broken.	Current caused by vibrations.
+	+	−	−
80	60	55	70
70	62	45	65
70	47	55	65
68	60		

Steel rod.

Current made.	Current caused by vibrations.	Current made.	Current caused by vibrations.
+	+	−	−
32	25	32	32
20	20	28	24
27	29	30	18
28	5	30	24
30	15	26	17

Table IV.—In all the previous cases the circuit was made while the rod was not vibrating.

Now the rod was first set in vibration, and the magnetizing circuit was made while it was vibrating, and the deflection noticed; when the rod came to rest, a tertiary current was excited by setting it again in vibration.

Here again more than one tertiary current was obtained, which could not have been due to the feebleness of the first vibration.

It will be noticed that the deflections obtained by the tertiary vibrations are opposite in direction to those obtained by the secondary ones, as was also the case in Table I.

On the iron rod the magnetizing helix was distant forty centimeters from the center; on the steel one fifteen.

Iron rod.		*Steel rod.*	
Current made while vibrating.	Current caused by vibration only.	Current made while rod was vibrating.	Current caused by vibrations only.
+	−	+	−
95	80	60	10
70	80	48	18
80	95	50	28
102	65–20	60	28–10
100	55–20	68	22–2
100	105	50	7
90	125		
122	105–25		

All the previous experiments were repeated with the rods at right angles to the meridian, but no noticeable difference in results was obtained. The conclusions are as follows:

1. When the primary magnetizing circuit was made and instantly broken, a tertiary current was excited by the vibrations which was less in amount and opposite in direction to the secondary current arising from the magnetism of the bar.

2. When the primary magnetizing circuit was made permanently, or in other words, while the bar was a permanent electro-magnet, the tertiary currents were in the same direction as the secondary; and in the case of soft iron uniformly greater.

3. When the rod formed the core of the magnetizing helix the tertiary currents were in the same direction as the secondary currents; and when the magnetizing circuit was broken they were, in the case of soft iron, greater than the secondary currents.

4. When the magnetizing circuit was made while the rod was in a state of vibration the tertiary currents were opposite in direction to the primary, and in several instances more than one was obtained at each trial.

No. XIII.—*Experiments on the Dissipation of Electricity by Flames;* by J. W. FEWKES.

By means of an electrometer made on the principle of Sir William Thomson's quadrant, I have been able to perform a few experiments in relation to the dissipation of small quantities of electricity by different kinds of flames.

These experiments were conducted with such small quantities of electricity as could be obtained by rubbing a vulcanite plate six inches square with a catskin. The sensitiveness of the electrometer to the electricity thus formed was very great. The experiments are given below.

Experiment 1.—An alcohol lamp, carefully insulated, was connected with the electrometer. The sections of the quadrant to which it was attached were then charged by means of the vulcanite plate, the opposite sections being at the same time in connection with the earth. The lamp was then carefully lighted. The spot of light, which had been deflected to the edge of the scale by the charge, quickly returned to the zero point, indicating a quick dissipation of the electricity by the flame.

Exp. 2.—The same conditions as those in Exp. 1 were observed, with the exception that a Bunsen burner was substituted for the alcohol lamp. The dissipation of electricity was the same as before, and took place, as near as could be observed, at the same rate as before.

Exp. 3.—I then substituted for the Bunsen flame a very fine jet of light, obtained by passing the gas through a finely pointed glass tube. The results obtained from this experiment

indicate that the rate of dissipation is in no respect related to the size of the flame.

Exp. 4.—The end of the wire connected with the quadrant was then placed so that when the gas was lighted the wire point would be in the flame. The quadrant was then charged and the gas turned on without being lighted. The spot of light had no movement, and gave no sign of any loss of electricity by the quadrant. An artificial current of air across the wire point likewise had no effect in dissipating the charge.

Exp. 5.—The end of the wire was then placed in the jet of an atomizer, the same conditions being observed as in Exp. 1. The fine globules of steam and water issuing from the atomizer had no effect in dissipating the electricity of the quadrant.

I also performed two very striking experiments, which seemed to have some bearing upon this subject. The instruments used were the same as in the former experiments and the manipulation was as follows:

Experiment 1.—Carefully insulate a wire communicating with the electrometer, and place its point within a few inches of the flame of an insulated Bunsen burner. Let the spot of light be at the zero point Electrify the vulcanite plate with the catskin and hold it at an equal distance from flame and wire point. It is very difficult under these conditions to sufficiently electrify the quadrant so as to produce any deflection of the spot of light.

Exp. 2.—Place the wire point in the flame and then hold the electrified vulcanite plate up to the flame as before. The spot of light immediately is violently deflected, indicating the presence of electricity in the quadrant. This change, however, is soon dissipated by the flame, and the spot quickly returns to the zero point.

These last experiments seem to indicate that the flame has a much greater attraction for the electricity of the vulcanite plate than the copper point of the wire. Hence the difficulty of charging the quadrant in the first experiment.

When, however, the wire is in direct communication with the flame, as in the second experiment, the flame and the quadrant are at the same potential, and the increase of electricity in the flame produces a corresponding deflection of the spot of light.

No. XIV.—*Polarization of the Plates of Condensers;* by A. S. Thayer.

It is well known that in polarization batteries, of which Plantè's battery is a type, a combination of the ions, resulting from electrolysis, takes place when the plates of the battery are connected, and a current results which slowly diminishes in

strength. In the case of condensers made with solid dielectrics the same diminishing current is observed, and the following experiments would seem to show that it might be due to an electrolysis or decomposition of the material separating the plates of tin-foil. The experiments consisted in placing condensers of various kinds in a circuit, through which a current was made to pass by two Bunsen's cells, and noting their changes. The plates of the condensers were of tin-foil and had an area of about fifteen square inches. The experiments were as follows:

(1.) The dielectric used was a sheet of dry glazed paper. The condenser could not be charged so as to give a perceptible discharge.

(2.) When a sheet of glazed paper, moistened with shellac, was substituted for the dry paper, the discharge was sufficient to send the light off the scale of the galvanometer, and continued for some minutes.

(3.) Dry goldbeaters' skin was used as a dielectric, and no deflection could be obtained.

(4.) The goldbeaters' skin, when moistened with shellac, gave a slowly diminishing deflection.

(5.) The dielectric was made by flowing the surfaces of the plates with a solution of wax and gasoline, and a slowly diminishing deflection was obtained.

(6.) The condenser used in (4) was tried again after the shellac had dried and again gave a diminished deflection less than the first deflection.

(7.) The condenser used in (5), when tried again after a day or two, did not again give a deflection.

(8.) Unglazed paper dry and oiled gave no deflection.

(9.) Glazed paper oiled gave a very slight deflection, and the galvanometer needle immediately returned to zero.

(10.) Glazed paper wet with water and covered with shellac gave the greatest deflection of all the dielectrics. The light was sent completely off the scale and was only brought back by shunting the galvanometer. The discharge also continued a long time.

(11.) The conducting power of some of the various dielectrics was tested. The goldbeaters' which had been covered with shellac transmitted no current after it had been allowed to stand for a week. Freshly oiled and dry oiled paper did not conduct at all. Glazed paper covered with shellac gave a deflection nearly off the scale. Glazed paper wet with water and covered with shellac transmitted a current sufficient to send the light entirely off the scale.

What these experiments directly go to show are, first, that condensers with moist dielectrics received a greater charge than

those made with dry, and second, that the better the dielectric conducted, the greater the charge the condenser was capable of receiving. From these facts it would seem that the slow discharge of these condensers was very probably due to polarization. The best condensers, as shown by the experiments, possessed dielectrics which were moist and possessed considerable conductivity. The dielectrics when dry scarcely conduct at all. Their conduction when moist must therefore have been mainly due to electrolysis, since liquids conduct electricity only in very small quantities without being decomposed. The electrolyte was therefore decomposed, and the re-combination of the products of decomposition caused the return current. An exact analogy is thus determined between the case of the lead plates and these condensers. Whether it is an analogy that would hold in the case of all condensers which slowly discharge themselves, is an interesting question.

SCIENTIFIC INTELLIGENCE.

I. Physics.

1. *Dielectricity of Insulators.*—M. L. Boltzmann has presented to the Vienna Academy of Sciences a paper on an experimental determination of the constant of dielectricity of insulating bodies. Faraday first observed the property of insulating solid bodies of being dielectric, that is, of increasing the capacity of a condenser by their presence between its two plates. Siemens, and later Gibson and Barkeley, have studied the phenomenon experimentally, while Clausius, Maxwell and Helmholtz have sought for the theoretical conditions. Representing by $\frac{dv}{dNi}$ and $\frac{dv}{dNe}$ the differential ratios of the potential on the inner and outer faces of the insulator, along the normal, the quotient $\frac{dv}{dNe} : \frac{dv}{dNi}$ will be the constant of dielectricity D. Neglecting the free electricity accumulated at the borders of the plates of the condenser, and calling n the distance of the two plates, or the thickness of the dielectric layer, the capacity of the condenser will be inversely proportional to $m-n+\frac{n}{D}$.

The measurement of the quantity of electricity was made by a Thomson's electrometer. The condenser was charged by a battery of 18 Daniell cells. The condenser was made of the form proposed by Kohlrausch. The insulators were hard caoutchouc, paraffine, sulphur and resin; then as imperfect insulators, stearine, glass and gutta-percha. The theoretical conclusions of Helmholtz

on the relation between the capacity of the condenser and the thickness of the insulating layer and plates were verified even when there were several insulating plates instead of one.

These experiments were made both with a momentary and with a continuous charge; both cases gave nearly equal capacities. From this we may conclude that the dielectric polarization is produced quite rapidly.

The author found as probable values of the constant of dielectricity the numbers for sulphur, 3·86; hardened caoutchouc, 3·15; resin, 2·55; paraffine, 2·32.

Maxwell arrived at the conclusion that the square root of the constant of dielectricity ought to equal the index of refraction. The following table shows that this law holds true quite within the limits of probable error:

	$\sqrt{D}$.	Index of refraction.
Sulphur,	1·960	2·060
Resin,	1·597	1·563
Paraffine,	1·523	1·536—1·516

Pogg. Annal., cli, 682; *Bib. Univ.*, cxcviii, 202. E. C. P.

2. *Flow of Saline Solutions through Capillary Tubes.*—M. T. Hübener finds that the velocity of flow of solutions in capillary tubes appears to depend solely on their weight and capillary adhesion. Thinking that besides the adhesion and weight, the intermolecular friction must be an important factor in determining the velocity, he compared a number of solutions brought to the same density, but having very different chemical compositions. The liquid was introduced into a vertical rectilinear glass tube 50 cms. in length and 1·75 cms. in diameter, having a capillary continuation about 60 cms. in length. The large tube presented two marks, and with a seconds-watch the time which was required for the level of the liquid to fall from one of these marks to the other, was accurately measured.

Operating in this way upon solutions of chloride, bromide and iodide of potassium, chloride of sodium and ammonium, with a density of 1·059 and a fixed temperature, the author ascertained that *the velocity* of flow of saline solutions is as much lower as the atomic weight of the salt dissolved is less. For the different binary bodies above indicated, it is the metal which has the greatest influence upon the velocity of flow, much more than the metalloid. The variations presented by the velocity from one body to another are as much more marked as the tube is more capillary and as the concentration of the solution is greater.

On comparing two solutions, of chloride of sodium and of ammonium, at 1·1058 density, the author arrived at the remarkable result that the times of flow of these two salts are found to be very sensibly proportional to their equivalents. From this experiment and from others analogous, extended also to the chlorides of the alkaline-earthy metals barium, strontium, magnesium, M. Hübener thinks it may be concluded generally, with a high degree of probability, that the velocities of flow of these

bodies in solution in water, to a certain degree of concentration, are in the same ratio as their equivalents.

The explanation of these facts is, according to M. Hübener, to be found in the circumstance that the molecules of substances which have a higher equivalent are larger, but, on the other hand, in less number, and consequently must give rise to less friction with the solvent in which they are held, thus communicating greater mobility to the solution.—*Bib. Univ.*, 197, 75; *Phil. Mag.*, xlviii, 77. E. C. P.

3. *Change of Volume by Fusion.*—Mr. R. MALLET presented to the Royal Society, at a recent meeting, a paper on the alleged expansion in volume of various substances in passing from a state of fusion to that of solidification. Since the time of Reaumur it has been assumed that certain metals, including bismuth, cast-iron, antimony, silver, copper and gold expand at or near their points of solidification. Recently certain cast-iron slags have been added to this number, but the following experiments seem to show that except in the case of bismuth the evidence is insufficient, and that with iron and slag it is quite erroneous. A conical vessel of wrought-iron about two feet in depth, 1·5 feet in diameter at the base, and with an open neck six inches in diameter, was weighed accurately, empty, and also when filled with water, level to the brim; the weight of its contents in water was thus obtained. After being dried, it was now filled to the brim with molten gray cast-iron, additions of molten metal being made to maintain the vessel full until it had attained its maximum temperature (yellow heat in daylight) and maximum capacity. The vessel and its contents of cast-iron when cold were weighed again, and thus the weight of the cast-iron obtained. From these data the specific gravity of the molten metal is readily obtained after applying the proper corrections. The final result for the molten cast-iron was found to be 6·650. The specific gravity of the solid cast-iron was determined in the usual manner and found to be 7·170, or greater than when in the molten state. This result was verified by heating two similar 10-inch shells (1·5 inches thick) to the same high temperature and filling one with melted iron. Its dimensions were then measured every 30 minutes during cooling. The second shell was employed to measure the permanent change of volume due to heating and cooling. After applying the proper and somewhat complicated corrections, it was found that there was no difference in volume of the two shells and that the solid ball was in perfect contact with the enclosing sphere, showing that no change in volume of the contents of the shell had taken place, which was further corroborated by the fact that the central portion was much less dense than the exterior, whereas if there was an expansion, the reverse would be the case.

To account for a solid floating on a liquid of a less specific gravity than its own, the author assumes the existence of a force which he calls the repellant force. The amount of this force depends on the relation between the volume and the effective sur-

face, or surface indented by the solid. It also depends on the difference of temperature of the solid and liquid.

In the case of lead, solid pieces float on the liquid metal, although the contraction on solidifying is here marked and well known. In fact the solid at 70° has a specific gravity of 11·361, while when melted its specific gravity is only 11·07. Floating or sinking takes place according to the relation between the volume and effective surface; thin pieces with large surface always floating, and vice versa.—*Nature*, 156. E. C. P.

II. Geology and Natural History.

1. *Reasons for some of the changes in the subdivisions of Geological time in the new edition of Dana's Manual of Geology;* by the Author.

(1.) *Archæan time.*—The first era in geological history is called, in the old edition of the Manual, *Azoic* time or age. The term Azoic was always objectionable, because it affirmed what was not proved. The discovery of the supposed animal fossil called *Eozoon*, shortly after the first edition was issued, led soon to the proposed substitution of *Eozoic* for Azoic. Those who received the suggestion with favor did not consider that if the so-called Azoic included an Eozoic era, it included a true Azoic also, an era of rocks and seas without life; for while the rocks and seas of the globe were above the temperature of boiling water, the Eozoic era could hardly have begun. The assumption that all those early rocks were Eozoic has nothing to favor it.

A general term for the whole era, free from hypothesis, was therefore needed. Murchison's term, *Bottom rocks*, was not satisfactory. *Archæan*, signifying simply *beginning-time*, was therefore used. Under *Archæan time*, there are the Azoic and the Eozoic ages, although their limits have not yet been marked out in the rocks of the world, and probably never will be, since the rocks are now crystalline, through metamorphism, and, with few exceptions, it cannot be learned whether life existed during their formation or not.

I pass now to the Lower Silurian era, which, in the new edition, is divided into (1) the Primordial or Cambrian, (2) the Canadian, and (3) the Trenton periods, instead of (1) the Potsdam or Primordial, (2) the Trenton and (3) the Hudson periods of the old edition.

(2.) *Primordial or Cambrian Period.*—This period in the new book has unchanged limits, except in the removal of the Calciferous sand-rock, the uppermost portion, whose fossils, as stated by Billings and the Geological Reports of Canada, are more nearly related to those of the following part of the Lower Silurian. The word *Potsdam* is dropped because the Potsdam sandstone is the least characteristic part of the formation. The term *Cambrian* is added, because the period is identical essentially with the Cambrian of the British geologists. The trilobites and other species

found within a few years in the Cambrian rocks of Britain—till then supposed to be almost destitute of organic remains—are all Primordial species, and so they were declared in the announcement of the facts by the discoverer. Lyell, in his Student's Elements of Geology (1871), makes the Cambrian extend to the Tremadoc slates, above the Lingula flags (or the equivalent of the Potsdam sandstone); and he states that all, from the Lingula flags to the Menevian of the Lower Cambrian inclusive, are embraced in the Primordial of Barrande. Geikie, in his edition of Jukes's Geology, draws the upper line below the Lingula flags, but says these flags "pass down conformably into the top of the Cambrian series, and are indeed closely linked with that series, both stratigraphically and paleontologically." Moreover, he adds, quoting Ramsay, the fossils of the Lingula flags differ to a great extent generically and almost wholly specifically, from those of the overlying groups."

The fossils of the supposed Huronian of Newfoundland, described by Billings, are referred in the work (p. 176) to the Primordial, because of the position and the occurrence of fossils. The Huronian is not a formation of known age. The original Huronian contains no fossils to mark off the era, and may yet turn out to be Silurian; and nearly all other regions or rocks called Huronian have been so pronounced on lithological evidence; which is no evidence at all, since the kinds of the Huronian are not confined to it.

(3.) *Canadian Period.*—The fact of the existence of an important Lower Silurian formation in Canada, near Quebec, abounding in fossils, and of about the age of the Calciferous sand-rock and the Chazy limestone, is mentioned, in the first edition of this work, as one of the discoveries of the Canadian Geological Survey, under Sir William Logan. The Reports of the Survey point out the close relations in fossils of the Calciferous sand-rock, Quebec group, and Chazy, and their rather wide separation from the overlying Trenton limestone, showing that they represent, naturally, a distinct period in the Lower Silurian era. This is called the Canadian, because the rocks are well displayed in Canada, and there the most important part of the facts respecting it were first brought to light.

(4.) *Trenton Period.*—The Trenton limestone and the Cincinnati (or Hudson River) group are closely alike in fossils, and the relation which exists between them (one dependent on physical changes) is best indicated by making the two represent subdivisions of one period; the differences being not equal to those distinguishing periods. The *Hudson River group* of the old edition of the Geology, was so named from the slates along the Hudson River. But when Logan gave his strong reasons for regarding these slates as of the age of his Quebec group, most American geologists felt it important that the name Hudson River, as applied to the beds overlying the Trenton limestone, should be changed. The change has been made to "Cincinnati" group in the recent geological reports on the surveys of States west of New York, and this term was accordingly adopted in the Geology.

(5.) *Upper Silurian, Oriskany Period.*—The Upper Silurian, in the first edition, closes with the Lower Helderberg formation, in accordance with the early suggestion of De Verneuil. It has always been doubted whether this places the top of the American Upper Silurian high enough in the series to correspond with the foreign; because so many Upper Silurian corals and other species of Britain occur higher up, that is, in the American Lower Devonian; and, moreover, remains of terrestrial plants exist in the Upper Silurian of Britain and Europe, but none had been found in North America. Professor Hall, in the third volume of his Paleontology (1859), presents reasons for transferring the Oriskany period from the Devonian to the Upper Silurian. He says, of these Oriskany sandstones, that "although usually very distinct from the limestones below, there are nevertheless localities where a passage occurs between the two rocks; and in such instances, some of the fossils, usually restricted to the lower beds [Lower Helderberg], pass into those above [Oriskany beds]. Instances of this kind occur in Maryland; and from the collections of the Canada Survey, by Sir William Logan, we are prepared to find in some parts of the Continent an intimate blending of these formations." During the preparation of the new edition of the Manual, I received word from Professor Hall that he still adhered to these views; and I accordingly made the change. Dr. Dawson has, within a few years, found fossil land-plants in beds at Gaspé, on the Gulf of St. Lawrence, containing Oriskany fossils; and now the earliest known species of this kind are from the Upper Silurian in America, as well as in Great Britain.

(6.) *Devonian Age, Catskill Period.*—In the Appendix to the first edition of the Geology, it is stated that recent observations made by E. Jewett and J. M. Way, in Delaware County, New York, tend to prove that the rocks of the so-called Catskill group are probably all Chemung. A note on this subject, by E. Jewett, is published in the American Journal of Science for 1862. But since then, no further facts on this important question have been published. Moreover, while the new edition of the Geology was in preparation, I received letters from Professor James Hall, of Albany, and from J. Peter Lesley, of Philadelphia (who was with Professor Rogers in the geological survey of Pennsylvania, and is now at the head of the new survey of the State), protesting strongly against the conclusion Mr. Jewett had announced. For these reasons, additional information appeared to be needed before making the Catskill beds Chemung. The Catskill formation of Pennsylvania has a thickness, according to Rogers, of 6,000 feet. Reference to the facts observed by Jewett and Way were inadvertently omitted.

(7.) *Quaternary Age, or Age of Man.*—This age in the new edition is merged in the Quaternary. The reasons for this, and for other modifications of statements introduced, are so fully given in the work, that they need not be here repeated.

The term Quaternary, which has been substituted for Post-tertiary, is in general use among European geologists. Moreover,

the geological work of the era was so widely different in its most prominent features from that of the Tertiary and preceding ages, that an independent term, implying no special relation to the Tertiary, was desirable; and such is Quaternary.

2. *The Winged Fruits of the Carboniferous genus Cardiocarpus.*—The genus Cardiocarpus was probably related to the modern Conifers of the *Welwitschia* type, as shown by the similarity of the fruit and also by the close relation of the leaves, if those called Cordaites belong, as both Geinitz and Newberry have independently remarked, to Cardiocarpus. The Welwitschia is an embryonic form of Conifer, it producing no leaves except the cotyledonous; but, while probably unlike *Cordaites* in its embryonic features, it shows what leaves and fruit are consistent with the type of Conifers.—*Dana's Manual of Geology*, pp. 328, 330.

3. *Coal of the Carboniferous era not made of bark.*—The suggestion has been made, in view of the many Sigillaria stumps hollowed out by decay and flattened stems of other trees, found in the Coal-measures, filled with shale or sandstone, that the vegetable debris from which the coal has proceeded was largely bark, or material of that general nature. But the occurrence of such stumps and stems outside of the coal-beds, while proof that the interior wood of the plants was loose in texture and very easily decayed, is no evidence that these trees contributed only cortical portions to the beds of vegetable debris. Moreover, the cortical part of Lepidodendrids (under which group the Sigillarids are included by the best authorities) and of Ferns also, is made of the bases of the fallen leaves, and is not like ordinary bark in constitution; and Equiseta have nothing that even looks like bark. This cortical part was the firmest part of the wood; and for this reason it could continue to stand, after the interior had decayed away,—an event hardly possible in the case of a bark-covered Conifer, however decomposable the wood might be. Further, trunks of Conifers are often found in the later geological formations, changed, *throughout* the interior, completely to brown coal or lignite.—*Ibid.*, p. 362.

4. *The ash of the better coals of the American Carboniferous age often derived wholly from the plants.*—The following are analyses of the ash of *Lycopods* (1, 2), *Ferns* (3, 6), *Equiseta* (7, 8), *Conifer* (9), a Moss of the genus *Sphagnum* (10), and a *Chara* (11).

	KO	NaO	CaO	MgO	Fe^2O^3	Mn^3O^4	Al^2O^3	PO^5	SO^3	SiO^2	Cl
1. Lyc. clavatum	31·90	2·68	4·13	5·89	6·00	..	22·20	7·30	3·55	13·01	..
2. Lyc. clavatum	25·69	1·74	7·96	6·51	2·30	2·53	26·65	5·36	4·90	13·94	3·13
3. Asplenium filix	45·5	5·2	7·9	7·4	1·5	..	..	20·0	6·8	2·2	4·6
4. Aspidium filix	39·80	5·31	18·74	8·28	0·97	..	..	2·56	5·40	4·38	14·72
5. Osm. spicant	23·65	3·33	4·09	6·47	1·17	..	..	1·76	1·29	53·00	5·82
6. Pteris aquilina	19·35	4·78	12·55	2·30	3·94	..	..	5·15	1·77	43·65	6·20
7. Eq. arvense	19·16	0·48	17·20	2·84	0·72	..	..	2·79	10·18	41·73	6·26
8. Eq. Telmateia	8·01	0·63	8·63	1·81	1·42	..	..	1·37	2·83	70·64	5·59
9. Pinus abies	12·84	5·64	58·27	2·81	1·60	*tr.*	*tr.*	2·60	1·60	12·55	2·06
10. Sphag. commune	8·02	12·40	3·17	4·92	6·35	*tr.*	5·89	1·06	4·33	41·69	12·09
11. Chara fœtida	0·85	0·44	95·35	0·99	0·67	..	..	0·54	0·42	1·22	0·16

Analysis 1, is by Ritthausen; 2, Aderholt; 3, A. Weinhold; 4, Struckmann; 5, 6, 9, Malaguti & Durocher; 7, 8, E. Wittig; 10, H. Vohl.; 11, Schultz-Fleet.

In the analyses that have been made of Lycopods, the amount of ash is 3·2 to 6 per cent in weight of the dried plant; of Ferns, 2·75 to 7·56 per cent; of *Equisetum arvense*, 18·71 per cent; of *Eq. Telmateia*, 26·75 per cent; of Conifers, mostly less than 2 per cent; of *Chara fœtida*, 31·33 per cent; of *Fungi*, 3·10 to 9·5 per cent; of Lichens, 1·14 to 17 per cent (the last in *Cladonia*), but mostly between 1·14 and 4·30 per cent. In *Lycopodium dendroideum*, Hawes, in his analyses, found 3·25 per cent of ash; in *L. complanatum*, 5·47 per cent, and in *Equisetum hyemale*, 11·82 per cent.

Lycopodium chamæcyparissus afforded Aderholt 51·85 per cent of alumina; or, when without spores, 57·36 per cent; while Ritthausen obtained 39·07 alumina for this species, and 37·87 for *L. complanatum*. In Lycopods, the silica constitutes 10 to 14 per cent of the ash. In the ash of Mosses have been found 8 to 23·58 per cent of potash, 4 to 16 of silica, 1·06 to 6·56 of phosphoric acid, 4·9 to 10·7 of magnesia. Among Ferns, the amount of ash, so far as determined, varies from 5 to 8 per cent.

The ash of *Fungi* affords 21 to 54 per cent of potash, 0·36 to 11·8 of soda, 1·27 to 8 of magnesia, 15 to 60 of phosphoric acid, and 0 to 15·4 of silica. Among *Lichens*, the ash of *Cladonia rangiferina* contains 70·34 per cent of silica; of other species, less, down to 0·9 per cent.

Trapa natans, of bogs, in Europe, affords 13 to 25 per cent of ash; and 25 per cent of this are oxide of iron (Fe^2O^3), with a little oxide of manganese. Of the ash of the fruit scales, over 60 per cent are oxide of iron.

Since, according to the average composition of Lycopods, the dried plant affords 5 pounds of ash to 100 of the plant, and 40 per cent of this are alumina and silica (27 alumina and 13 silica), these two ingredients make up 2 per cent of the plants. Ferns, with the same amount of ash, afford, as the average, 27 per cent of silica, with no alumina. Equiseta afford, on an average, 20 per cent of ash, and 50 per cent of this may be silica. Supposing, now, that Lycopods (*Lepidodendrids*, etc.) afforded one-half the material of the coal-beds, and the other plants the rest, and that the silica and alumina of the former averaged 40 per cent, and of the latter only 27 per cent, this being all silica, then the amount of these ingredients afforded by the vegetation would be 1·66 per cent of the whole weight when dried. This would make the amount of silica and alumina, in the bituminous coal made from such plants (supposing three fifths of the material of the wood lost in making the coal, as estimated on page 363), 4 per cent; and the whole amount of ash about 4·75 per cent. At the same time, the ratio of silica to alumina would be nearly 3 to 2.

Now many analyses of the bituminous coal of the Interior basin have obtained not over 3 per cent of ash, or impurity, although the general average, excluding obviously impure kinds, reaches 4·5 to 6 per cent; being, for the coals of the northern half of Ohio, 5·12, and for the southern half 4·72 per cent.

It hence follows that (1) the whole of the impurity in the best coals may have been derived from the plants; (2) the amount of ash in the plants was less than the average in modern species of the same tribes; (3) the winds and waters for long periods contributed almost no dust or detritus to the marshes; and (4) the ash, or else the detritus, was greatest in amount toward the borders of the Interior marsh-region. In that era of moist climate

and universal forests, there was almost no chance for the winds to gather dust or sand for transportation.—*Ibid.*, pp. 365, 366.

5. *Fossil plants of the Carboniferous age in the Protogine of the Alps.*—The hypothesis has been urged of late years that the gneissic, hornblende, chloritic and other allied metamorphic rocks of New England and the rest of the world are exclusively *pre-Silurian;* and the fact that the protogine (chloritic gneiss) of the Alps had been made, by one geologist, pre-Silurian has been regarded by the supporter of the hypothesis as the best evidence in its favor: though if good truth, it really proves nothing with reference to the age of the protogine of the rest of the world. But, in Nature, of July 30th, occurs the following announcement.

"Prof. Schimper, of Strasburg, in a paper read before the Botanical Congress at Florence, claims to have discovered a fossil plant in "protogine," a rock hitherto considered of igneous origin, which occurs in the form of erratic blocks on the sides of Mont Blanc and in the plains of Piedmont. The specimen, which was collected by M. Sismonda, and is preserved in the Museum of the Turin University, has been identified by Prof. Schimper as *Annularia sphenophylloides*, a plant, perhaps aquatic, widely distributed in the coal strata of Mont Blanc."

6. *The proposed genus Anomalodonta of Miller identical with the earlier Megaptera of Meek.*—In the first issue of the Cincinnati Journal of Science I observe that the editor, Mr. S. A. Miller, proposes a new genus under the name of *Anomalodonta*, to include a group of shells allied to *Ambonychia* of Hall. These shells constitute a very interesting type, evidently belonging to the family Aviculidæ. Like *Ambonychia*, they are destitute of an anterior wing, but have, posteriorly, a very large one, which gives the shell a trigonal outline. Mr. Miller's type specimens show them to have a broad striated area, such as *Myalina* possesses, with, at the anterior end of the hinge, an oblique fold or ridge, not properly a tooth perhaps, together with a corresponding depression, in one or both valves. We see just such characters in the broad forms of *Myalina*, as for example in *M. ampla* of Meek and Hayden, so that, although generically distinct from Mr. Miller's shell, there seems to be little to distinguish the latter except outline and radiating costæ, unless he is right in stating that his shell has a large impression of the anterior adductor muscle, which, in view of the affinities of the shell, is very improbable, especially so large an impression and in so low a position as he has figured it. Compared with *Ambonychia*, the hinge of his shell differs materially in wanting the two anterior well-defined teeth of that genus, and also its three elongated posterio-lateral teeth.

This genus *Anomalodonta* was proposed for a group of shells identical with the one for which Meek & Worthen proposed the subgeneric name of *Megaptera* in the Proceedings of the Chicago Academy of Science in 1866. Mr. Miller himself states that his proposed genus will include *Ambonychia* (*Megaptera*) *alata* Meek (Ohio Paleontology, vol. i, p. 131), of which he says Mr. U. P. James

has specimens showing the same hinge characters. He also states that his proposed genus will include, among others, *Megaptera Casei* of Meek & Worthen, which is the type species of *Megaptera*. In short, he proposes a new genus to include the type of a previously established one, together with another species of the same, described by one of the authors of the forenamed genus. *Megaptera* has unquestionable priority, and must stand before Mr. Miller's name and all others also, unless, however, the name *Megaptera* may be objected to on account of its previous use by Dr. Gray for a genus of whales. Even in that case Mr. Miller's name could not stand, because Mr. Meek, in the Proceedings of the Philadelphia Acad. Nat. Sci., 1872, proposed to use the name *Opisthoptera* as a substitute in case such objection should arise. Indeed, I am informed by a friend that Mr. Meek in his work, now in manuscript, has decided to use the name *Opisthoptera Casei* and *O. alata* for *Megaptera Casei* and *M. alata*.

The fact that Meek & Worthen did not describe the hinge of *Megaptera*, which was unknown to them, in their generic diagnosis, has no bearing upon the question of priority. If it were otherwise, few genera of fossils would be allowed to stand after the discovery of more perfect specimens than the original workers possessed. Nor does the fact that Meek & Worthen called theirs a sub-genus affect the question, for the rule covers such cases as well as others, and the use of such a designation under similar circumstances indicates on the part of an author a degree of caution that all true naturalists will approve.

Further, upon comparing Mr. Miller's description of his species *gigantea* with Meek's *alata*, I am unable to find that the former possesses characters sufficient to separate it specifically from the latter. Mr. Miller seems to have relied solely upon the large size and the preservation of concentric striæ of his shell to separate it from *M. alata*. As Mr. Meek distinctly mentions the existence of the striæ in his shell, Mr. Miller's species seems to be reduced to the very insufficient ground of size alone. C. A. W.

7. *On the Quaternary containing the New Brunswick fossil Cetacean; on Niagara Coral reefs; and on Niagara fossils in trap;* by Rev. D. HONEYMAN. (From a letter to J. D. Dana, dated Halifax, N. S., Provincial Museum, Aug. 6.)—I have just returned from the locality of the fossil Cetacean, in New Brunswick. I have examined the bed of the fossil and found it a clay covered with from twelve to twenty feet of sand and gravel. In the bed I found well-preserved shells, e. g., *Balanus Hameri*, *Buccinum undatum*, *Fusus tornatus*, *Natica Grœnlandica*, *N. helicoides*, *Leda caudata*, *L. truncata*, *Mya arenaria*, *M. truncata*, *Tellina Grœnlandica*, *T. proxima*, and *Saxicava rugosa*. This is one of a series of cuttings extending from Bathurst toward Canada. I have collected the above and others from cuttings, six in number, at intervals for a distance of thirty miles.

The Rev. Mr. Paisley described one of these cuttings in the Canadian Naturalist. This occurs about three miles north of Bath-

urst at Somersetvale. It is the first cutting north of the river Tittigouche. The fossils are here in the greatest variety and abundance. The following measurements give the approximate height of the bed above the present sea level: The height of the iron bridge over the Tittigouche is 60 feet; the foundation is about ten feet above sea level; the height of the top of the clay in the cutting is about seven feet above the height of the bridge; total, 77 feet. I found the vertebra of a mammal with the shells in this cutting. As Dr. Gilpin is at present in the country, it has not been examined.

I also examined two other noteworthy localities; one of these is Belledune. On the shore I found Niagara limestone strata, having an abundance of corals. I collected fine specimens of *Favosites Gothlandica*, *Halysites catenulata*, species of *Cyathophyllum*, or allied.

At Cape Bon Ami, I unexpectedly came upon a fine exposure of strata of the same age—great ancient coral reefs, replete with magnificent specimens of corals, which were found almost falling out of the rocks, and scattered on the shore. I collected magnificent specimens of *Favosites Gothlandica*, *F. basaltica*, with specimens of *Zaphrentis*, sp. ?, *Orthoceras*, sp. ?, *Strophomena depressa*, *Atrypa reticularis*, *Athyris nitida*, *Rhynchonella*, sp. ? *Orthis*, sp. ? and crinoidal joints.

Associated with this is a great dike of intrusive trap. It is both basaltiform and amygdaloidal; geodes and amygdules are abundant. The minerals are agates, zeolites, and calcite. In examining the amygdules, a moniliform specimen attracted my attention; on detaching it and examining it with a magnifying glass, I found it to be a specimen of *Favosites Gothlandica*, which had dropped out of the penetrated Niagara limestone strata, had become imbedded in this ancient *lava*, and formed an *amygdule* in common with the agates, etc. On examining the mass of amygdaloidal trap at a short distance from its line of contact with the fossiliferous strata, I was fortunate enough to find another specimen more firmly imbedded in the trap than the preceding, so much so as to be wholly incorporated with it, there being small pieces of agate imbedded in the hand specimen of enclosing trap secured. The exposed section of coral shows structure as distinct as any figured on page 239 of your Manual. I also found a detached specimen of *Cyathophyllum* with trap attached. Well-preserved fossils embedded in trap are new in my experience. These indicate conditions which some might be disposed to question. They show that the trap was soft fluid when the corals were imbedded. They show that the trap was post-Upper Silurian. They show that the existing heat in that part of the mass was not sufficient to metamorphose the organism,—or that corals have a greater power of resistance than other organic remains.

8. *Descriptions of new species of Goniatidæ, with a list of previously described species;* by JAMES HALL. 4 pp. 8vo. Printed in advance of the 27th Report on the State Museum of Natural History. (Received from the author in May last.) The species described are *Goniatites complanatus* Hall, (referred, from a

poor specimen, in the 4th volume of the N. Y. Paleontology, to *Clymenia*); and a variety *perlatus*, of the same species, from the Chemung in Cortland Co., N. Y.; *G. unilobatus* Hall, from the Hamilton shales, Cayuga Lake; *G. simulator* Hall, from the Chemung, near Ithaca; *G.* (*Clymenia?*) *Nundaia* Hall, same locality; *G. Chemungensis*, var. *equicostatus*, from the Chemung of Athens, Pa.

9. *Preliminary Report on the First Season's Work of the Geological Survey of Yesso;* by B. S. LYMAN. 46 pp. 8vo. Tokei, 1874.—Besides other topics, the Report gives information respecting the position of the Yesso coal, speaking of it as brown coal, probably of Tertiary age. Springs of thick oil, or rather black tar, come from deposits, perhaps Tertiary, in Washinoki, Yamukushinai and Idzumisawa.

10. *Reply to Dr. T. Sterry Hunt;* by F. A. GENTH. (Read before the American Philosophical Society, July 17, 1874.)—Dr. T. Sterry Hunt has published, in the Proceedings of the Boston Society of Natural History, vol. xvi, March 4th, 1874, an article, entitled: "*On Dr. Genth's Researches on Corundum and its associated minerals*," in which he charges me—*in common with many others*—of having *fallen into errors* and of having been *led to conclusions wholly untenable*, for *lack of a clear understanding as to replacement, alteration and association in the mineral kingdom.*

He then gives an outline of the manner in which the various alterations in a mineral species may take place, by replacement, envelopment and epigenesis, with examples for each, and dwells at more length upon the fallacy of considering the alterations of many minerals and rock masses as the *result of an epigenic process; a doctrine which has been embodied in the dictum of Professor Dana:* "*regional metamorphism is pseudomorphism on a broad scale.*"*

* Mr. Hunt knows that this "dictum of Prof. Dana," as he calls it (which is a clause in a sentence of a book-notice written by me in this Journal, xxv, 445, 1858), does not represent at all the views on metamorphism that I have held for the past twelve years; for, besides having given him a copy of my Geology in 1862, I took especial pains, in 1872, in my review of his American Association Address, to refer him to the chapter on Metamorphism in the work, that he might read and appreciate the grossness of the misrepresentation. He knows, also, that the statement in the preceding part of the same sentence in his paper, that the "advocates of the doctrine of transmutation have not hesitated to assert, upon this supposed evidence [that of facts connected with replacement and envelopment], the conversion of almost every mineral species into some other, and to extend this view to rock-masses, declaring that the *greater part of all the so-called metamorphic or crystalline rocks are the result of an epigenic process*"—and another, in the next sentence following this, that "*the advocates of this doctrine maintain that a mass of granite or diorite may be converted into serpentine or limestone, and that a limestone may be changed into granite or gneiss, which may in its turn become serpentine*" (both of which he put forth in the same Address), have been shown by me to be untrue. For I assured him in the review and its sequel that I had never held the extravagant doctrine thus attributed to myself and others; that the idea of such transformations as are above enumerated had never occurred to me until found in his foolish charge against "Gustaf Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others," in his Address; and I demon-

He then refers briefly to the results of my investigation on corundum, in which I have shown that by "epigenic" pseudomorphism this mineral has been altered into numerous more complex species and rock masses—and winds up by stating that he *not only has carefully studied my paper*, but has also *examined the extensive collection of species upon which my conclusions were based*, and that—*all the phenomena in question are nothing more than examples of association and envelopment, and that the corundum-bearing veins had their parallels in the granitic veins with beryl and tourmaline in the White Mountain rocks, and the calcareous veinstones with apatite, pyroxene, phlogopite and graphite of the Laurentian rocks.*

I may be permitted to say a few words in reply to Dr. Hunt's assertion, than I had fallen into errors and had been led to wholly untenable conclusions.

When I had the good fortune to obtain a few years ago the *first real* pseudomorph after corundum—the spinel from India, and afterward brought together numerous specimens of analogous alterations, showing from the *same* locality crystals of corundum without any, and others representing all stages of alteration from a thin coating to the complete disappearance of every vestige of corundum, and when I proved that such changes have resulted in the conversion of corundum into about two dozen mineral species; I could not understand how any unprejudiced mind could arrive at any other conclusions, but that these extraordinary occurrences which I have described were the result of epigenic pseudomorphism.

This opinion has been adopted almost without exception by all who have had an opportunity to examine my specimens, or who have studied my paper. If Dr. Hunt differs from me, I certainly will not deny to him the right to believe what suits his own notions; but when he boldly charges me with having committed errors, I want better proofs than a repetition of his views, with which we were familiar long ago. He certainly has not a single fact which could show the fallacy of my conclusions, or he would have produced it.

The corundum alterations have nothing in common with the Fontainebleau crystals, or with stanniferous orthoclase, or the green

strated that all writers on pseudomorphism, with but one or two exceptions, would repudiate it as strongly as myself. Nevertheless, he finds it to suit his purpose to repeat the citation and the perverted statement without a qualifying remark.

My review of Mr. Hunt's Address, and the sequel, in which these points are fully discussed, are contained in volumes iii of this Journal, page 86, iv, page 97, 1872, and v, page 312, 1873. I state that the regional metamorphism in which serpentine—a hydrous magnesian rock—is formed, has been regarded by writers on pseudomorphism, myself included, as pseudomorphism on a broad scale; and that other hydrous magnesian rocks may come under the same category; and I still so believe with regard to most beds of serpentine. But this is no ground for the assertions of Mr. Hunt above cited.

Solitary in his views on pseudomorphism and defiant of facts, he shows, by his efforts to fix absurd opinions on others, a degree of desperation in argument that is, fortunately, very uncommon. His way of carrying a point is strongly in contrast with Dr. Genth's faithful work.—J. D. D.

and red tourmalines from Paris, Me., or the beryls filled with orthoclase, or the zircon and galenite filled with calcite, and cannot be explained *rationally* as examples of *association* and *envelopment.*

To give strength to his statements, however, Dr. Hunt says that he had "*examined*" with me "*the extensive collection of specimens upon which my conclusions were based.*" When Dr. Hunt favored me with a visit, I was in hope that he *would examine* my specimens, but his time was so short that he saw only about one-third of them, and the "*examination*" (*!?*) of these was finished in about *five minutes.*

As to his last sentence, I must confess that I am unable to discover the least parallelism between the corundum-bearing veins and the granitic veins, with beryl and tourmaline, so common in the White Mountains, and the calcareous veinstones with apatite, pyroxene, phlogopite and graphite of the Laurentian rocks;—but can see in the former nothing but the product of a partial, and in many instances of a pretty thorough alteration of the original corundum into micaceous and chloritic schists or beds, or, as Professor Dana would express it, "*a pseudomorphism on a broad scale.*"

University of Pennsylvania, July 4th, 1874.

11. *Note on the Enemies of Difflugia;* by J. LEIDY. (Proc. Acad. Sci. Philad., p. 75, 1874.)—Prof. Leidy remarked that in the relationship of *Difflugia* and *Amœba* we would suppose that the former had been evolved from the latter, and that its stone house would protect it from enemies to which the *Amœba* would be most exposed. The *Difflugia* has many enemies. I have repeatedly observed an *Amœba* with a swallowed *Arcella*, but never with a *Difflugia.* Worms destroy many of the latter, and I have frequently observed them within the intestine of *Nais*, *Pristina*, *Chætogaster*, and *Æsolosoma.* I was surprised to find that *Stentor polymorphus* was also fond of *Difflugia*, and I have frequently observed this animalcule containing them. On one occasion I accidentally fixed a *Stentor* by pressing down the cover of an animalcule cage on a *Difflugia*, which it had swallowed. The *Stentor* contracted, and suddenly elongated, and repeated these movements until it had split three-fourths the length of its body through, and had torn itself loose from the fastened *Difflugia.* Nor did the *Stentor* suffer from this laceration of its body, for in the course of several hours each half became separated as a distinct individual.

12. *Remarks on the Revivification of Rotifer vulgaris;* by J. LEIDY. (Ibid, p. 88).—Prof. Leidy remarked that during the search for Rhizopods, having noticed among the dirt adhering to the mosses in the crevices of our pavements many individuals of the common wheel-animalcule, *Rotifer vulgaris*, he had made some observations relating to the assertion that they might be revivified on moistening them after they had been dried up.

Two glass slides, containing beneath cover glasses some dirt, exhibited each about a dozen active living Rotifers. The glass

slides were placed on a window ledge of my study, the thermometer standing at 80°. In the course of half an hour the water on the slides was dried up and the dirt collected in ridges. The next morning, about twelve hours after drying the slides, they were placed beneath the microscope. Water was applied and the materials on the slides closely examined. On each slide a number of apparently dried Rotifers were observed. These imbibed water and expanded, and some of them in the course of half an hour revived and exhibited their usual movements, but others remained motionless to the last.

The same slides were again submitted to drying, and from one of them the cover glass was removed. They were examined the next day, but several hours after moistening them only two Rotifers were noticed moving on each slide.

I next prepared a slide on which there were upward of twenty actively moving Rotifers, and exposed it to the hot sun during the afternoon. On examination of the slide the following morning, after moistening the material, all the Rotifers continued motionless, and remained so to the last moment.

From these observations it would appear that the Rotifers and their associates became inactive in comparatively dry positions and may be revived by supplying them with more moisture, but when the animals are actually dried they are incapable of being revivified. Moisture adheres tenaciously to earth, and Rotifers may rest in the earth, like the *Lepidosiren*, until returning waters restore them to activity.

13. *Notice of some new Fresh-water Rhizopods;* by J. LEIDY. (Ibid, p. 77.)—Prof. Leidy remarked that besides the ordinary species of *Amœba*, which he had observed in the vicinity of Philadelphia, he had discovered what he suspected to be a new generic form. It has all the essential characters of Amœba, but in addition is provided with tufts of tail-like appendages or rays, from which he proposed to name the genus OURAMŒBA.

The rays project from what may be regarded as the back part of the body, as the animal always moves or progresses in advance of the position of those appendages. The rays are quite different from pseudopods, or the delicate rays of the Actinophryens. They are not used in securing food, nor is their function obvious. The *Ouramœba* moves like an ordinary *Amœba*, and obtains its food in the same manner. The tail-like rays are not retractile, and they are rigid and coarse compared with those of Actinophryens. They are simple or unbranched, except at their origin, and they are cylindrical, of uniform breadth, and less uniform length. When torn from the body they are observed to originate from a common stock attached to a rounded eminence.

Several forms of the *Ouramœba* were observed, but it is uncertain whether they pertain to one or to several species. One of the forms had an oblong ovoid body about $\frac{1}{8}$th of a line long and $\frac{1}{12}$th of a line broad. The tail-like rays formed half a dozen tufts, measuring in length about the width of the body. The latter

was so gorged with large diatoms, such as *Navicula viridis*, together with desmids and confervæ, that the existence of a nucleus could not be ascertained. The species may be distinguished with the name of OURAMŒBA VORAX.

A second form, perhaps of a different species, moved actively and extended its broad pseudopods like *Amœba princeps*. When first viewed beneath the microscope it appeared irregularly globular and about $\frac{1}{14}$th of a line in diameter. It elongated to the $\frac{1}{6}$th of a line, and moved with its tail-like appendages in the rear. These appendages formed five tufts about $\frac{1}{75}$th of a line long. The interior of the body exhibited a large contractile vesicle and a discoid nucleus. This second form may be distinguished with the name of OURAMŒBA LAPSA.

Another *Ouramœba* had two comparatively short tufts of rays, and a fourth, of smaller size than the others, had a single tuft of three moniliform rays.

It is possible that *Ouramœba* is the same as the *Plagiophrys* of Claparede, though the description of this does not apply to that.

Plagiophrys is said to be an Actinophryen, furnished with a bundle of rays emanating from a single point of the body, but the rays are described as of the same kind and use as those of *Actinophrys*. *Plagiophrys* is further stated to be provided with a distinct tegument like *Corycia* of Dujardin, or *Pamphagus* of Bailey, but the body of *Ouramœba* is as free from any investment as an ordinary *Amœba*, and the rays are fixed tail-like appendages, with no power of elongation or contraction.

The species of *Ouramœba* were found among desmids and diatoms, on the surface of the mud at the bottom of a pond, near Darby Creek, on the Philadelphia and West Chester Railroad.

Two of the commonest species of *Difflugia* of our neighborhood I had until recently confounded together as *D. proteiformis*, and, perhaps, the two forms may be included under the latter name in Europe. In one the mouth is deeply trilobed, and the animal is usually green with chlorophyl globules. In the other the mouth is crenulate, usually with six shallow crenulations, and the animal is devoid of chlorophyl. The former is usually the smaller, and may be distinguished with the name of D. LOBOSTOMA; the latter may be named D. CRENULATA.

In an old brick pond, on the grounds of Swarthmore College, Delaware County, among *Difflugia pyriformis*, *D. spiralis*, *D. corona*, *D. acuminata*, and others not yet determined, there occurs an abundance of a large species, apparently undescribed. It is sometimes the fourth of a line in length, and is compressed pyriform, but is quite variable in its relation of length to breadth, and in the shape of the fundus of the shell. This is often trilobate, but from the non-production of one or more or all the lobes, differs in appearance in different individuals. The animal is filled with chlorophyl grains, from which it might be named D. ENTOCHLORIS.

Another large *Difflugia*, allied to *D. lageniformis*, is not unfre-

quent about Philadelphia. The shell is beautifully vase-like in shape. It has an oval or sub-spherical body with a constricted neck, and a recurved lip to the mouth. The body of the shell opposite the mouth is acute and often acuminate. The animal contains no chlorophyl. One shell measured $\frac{1}{6}$ of a line long by $\frac{1}{8}$ of a line broad; another measured $\frac{1}{4}$ of a line long by $\frac{1}{7}$ of a line broad. The species may be named D. AMPHORA.

A Difflugian, found in a spring on Darby Creek, is interesting, from its transparency, which allows the structure of the animal to be seen in all its details. The investment is membranous and apparently structureless. The soft granular contents occupy about one-half of the investment, and are connected with this by long threads. The pseudopods are protruded in finger-like processes. The form of the animal is compressed ovoid, with the narrow pole truncate and forming the transversely oval mouth. It is probably the species *Difflugia ligata*, described by Mr. Tatem, of England. Its length is about $\frac{1}{33}$d of a line. The character of the investment is so different from that of ordinary Difflugians that the species may be regarded as pertaining to another genus, for which the name of CATHARIA would be appropriate.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Change of level in the Great Salt Lake.*—On Tuesday, July 21st, under the direction of Dr. J. R. Park, of the Deseret University, a monument or pillar was placed near the shore of Great Salt Lake at Black Rock, to indicate the rise and fall of the water.

The necessity of such an indicator has been long apparent to many of the more intelligent observers. Several years since, Prof. Henry, of the Smithsonian Institution, Washington, D. C., wrote to Mr. B. Young on the subject, and requested that a fixed monument might be placed in some convenient part of the lake, furnished with divisions in feet and inches, by which the height of the water could be periodically measured. We are not aware that any notice was taken of the Professor's communication. This request Prof. Henry renewed last March, in a communication to Dr. Park, soliciting, at the same time, some definite information as to the past and present height of the water of the lake.

In common with all others who have given the subject any consideration, the Professor regarded this isolated body of water as an immense rain gauge, indicating from year to year the changes in the mean quantity of water that falls in the region constituting the basin of which it forms the bottom. The changes are indicated both by the depth of the water and the varying extent of ground over which it is spread.

It is apparent to all persons conversant with the meteorology of the Great Basin, that rainfall is a term of comparatively trivial import in this altitude. Except in the autumn, when the waters are at low stage, no rains occur here that materially affect the streams from which Salt Lake derives its volume. The evapora-

tion is so great that even the extraordinary succession of heavy rains that have fallen during the present month (July) has had no perceptible effect upon the waters of the lake—not even to compensate for the amount of evaporation during the month. On the contrary, such is the intensity of heat and the absence of moisture from our atmosphere, that the waters of the lake have steadily subsided, showing, from the best data obtainable, a fall of about fourteen inches, and this mostly in the present month. Admitting a total surface area of 1,300,000 square feet, this would give a ratio of evaporation of one millionth part of an inch to every square foot of surface. The season of high waters, then (when the rivers and streams are swollen by the melting of the deep snows in the mountains), is the only period at which the falling of the lake is arrested; the rise continues until an equilibrium is formed between the surface exposed and the amount of evaporation which goes on from every square foot of the surface.

It is to be regretted that there are no means of definitely ascertaining the variations of the lake since the settlement of the valley in 1847. It is generally conceded by old residents, however, that the lake is now some twelve or fourteen feet higher than then.

Until the spring of 1852 there was no perceptible permanent rise; the increase from the high waters of the spring, and the subsidence during the autumn, being about equal, and the extreme variation some fifteen inches. From the autumn of 1852 to the autumn of 1856 the snows fell heavier in the mountains, and produced a corresponding increase in the volume and extent of the lake; exhibiting, in the latter year, a rise of some six feet above the lowest stage of 1852. From 1856 to 1861 a gradual subsidence of the waters took place, the fall in 1861 reaching a mark two feet below its lowest stage prior to 1852. The year 1861 will be remembered by the farmers of Utah as a season of unprecedented drought, the crops being seriously injured from lack of water for irrigation purposes. A contraction of the lake's surface was also observable to about three fourths of its dimensions in 1852; and more than half of this area was less than five feet in depth. Captain Stansbury, in 1850–51, reports its greatest depth as fifty-six feet.

In the spring of 1862 the lake began to extend its area and continued to rise until 1868, when it had reached a point twelve feet higher than the lowest stage of 1861, with an area estimated at one and a half times that of 1861.

Since 1868, up to the present time, the rise and fall have been about equal, the lake holding its own, with a slight increase and an extreme variation of about two feet.

The data we have presented above show conclusively an irrepressible determination of the waters to rise. The mountain streams are steadily enlarging. The humidity of the atmosphere annually increases as the area of cultivation in the valleys becomes greater, and, as a consequence, the evaporation less. Tens

of thousands of acres of farming, meadow and pasture lands have been submerged along the eastern and northern shores of the lake. Many square miles of valuable lands as yet available and occupied by the farmer, skirting the lake, would be completely submerged should the waters rise but a few inches above the average level of the past five years.

There is one fixed mark corroborative of the immense increase in the lake during the past decade. Prior to 1861, Black Rock was connected with the mainland by a roadway of black limestone, crushed and regularly turnpiked, as if by design. Over this road, rising in the center about two feet above the waters, the salt manufacturers of earlier days—Mr. White and others—having salt vats and flues on the shores of Black Rock, transported their saline products. That self-same roadway, yet plainly discernible, is now twelve feet under the surface of the Great Salt Lake. Essentially it is a submarine turnpike!

It is no mere conjecture that the lowland farmers along the shores of the Great Salt Lake may some day find themselves in the predicament of the demure Hollander—compelled to resist, by earthworks, the encroachments of salt water, or submit to the retiring process of inundation.—*Utah Mining Gazette*, July 25.

2. *On the Physical Cause of Ocean-Currents;* by JAMES CROLL, of the Geological Survey of Scotland.—In a lecture at the Royal Institution, and also in the Athenæum, Nature, Philosophical Magazine, and other quarters, Dr. Carpenter has been advancing a somewhat plausible-looking objection to my views in reference to under-currents. As this objection bears upon a point which in my last communication I omitted to consider, perhaps you will permit me, through your columns, briefly to refer to it. The objection in question, as stated in Dr. Carpenter's own words, is as follows:

"According to Mr. Croll's doctrine, the whole of that vast mass of water in the North Atlantic, averaging, say, 1500 fathoms in thickness and 3600 miles in breadth, the temperature of which (from 40° downward), as ascertained by the 'Challenger' soundings, clearly shows it to be mainly derived from a polar source, is nothing else than *the reflux of the Gulf-stream.* Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf-stream to be about 1-900th that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf-stream returns into the Mid-Atlantic circulation, only a branch of it going on to the northeast, the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison a mere rivulet (the northeast motion of which, as a distinct current, has not been traced eastward of 30° W. long.) seems still more obvious."

The objection seems to me to be based upon a series of misap-

prehensions: (1) that the mass of cold water 1500 fathoms deep and 3600 miles in breadth is in a state of motion toward the equator; (2) that it cannot be the reflux of the Gulf-stream, because its sectional area is 900 times as great as that of the Gulf-stream; (3) that the immense mass of water is, according to my views, set in motion by the Gulf-stream.

I shall consider these in their order. (1) That this immense mass of cold water came originally from the polar regions I, of course, admit, but that the whole is in a state of motion I certainly do not admit. There is no warrant whatever for any such assumption. According to Dr. Carpenter himself, the heating power of the sun does not extend to any great depth below the surface; consequently there is nothing whatever to heat this mass but the heat coming through the earth's crust. But the amount of heat derived from this source is so trifling, that an under-current from the Arctic regions, far less in volume than that of the Gulf-stream, would be quite sufficient to keep the mass at an ice-cold temperature. Taking the area of the North Atlantic between the equator and the tropic of Cancer, including also the Caribbean Sea and the Gulf of Mexico, to be 7,700,000 square miles, and the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, we find that the total quantity of heat derived from the earth's crust by the above area is equal to about 88×10^{15} foot-pounds per day. But this amount is equal to only $\frac{1}{894}$ that conveyed by the Gulf-stream, on the supposition that each pound of water carries 19,300 foot-pounds of heat. Consequently an under-current from the polar regions of not more than $\frac{1}{35}$ the volume of the Gulf-stream would suffice to keep the entire mass of water of that area within 1° of what it would be were there no heat derived from the crust of the earth; that is to say, were the water conveyed by the under-current at 32°, internal heat would not maintain the mass of the ocean in the above area at more than 33°. The entire area of the North Atlantic from the equator to the arctic circle is somewhere about 16,000,000 square miles. An under-current of less than $\frac{1}{17}$ that of the Gulf-stream coming from the Arctic regions would therefore suffice to keep the entire North Atlantic basin filled with ice-cold water. In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions either in the form of under-currents, or in the form of a general under-flow as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water; for there is no source from which this influx of water can derive heat, save from the earth's crust. But the amount thus derived is so trifling as to produce no sensible effect. For example, a polar under-current one-half the size of the Gulf-stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold tem-

perature. Internal heat would not be sufficient under such circumstances to maintain the mass 1° Fahr. above the temperature it possessed when it left the polar regions.

(2) But suppose that this immense mass of cold water occupying the great depths of the ocean were, as Dr. Carpenter assumes it to be, in a state of constant motion toward the equator, and that its sectional area were 900 times that of the Gulf-stream, it would not therefore follow that the quantity of water passing through this large sectional area must be greater than that flowing through a sectional area of the Gulf-stream; for the quantity of water flowing through this large sectional area depends entirely on the rate of motion.

(3) I am wholly unable to understand how it could be supposed that this under-flow, according to my view, is set in motion by the Gulf-stream, seeing that I have shown that the return under-current is as much due to the impulse of the wind as the Gulf-stream itself.

I am also wholly unable to comprehend how Dr. Carpenter should imagine, because the bottom-temperature of the South Atlantic happens to be lower, and the polar water to lie nearer to the surface in this ocean than in the North Atlantic, that therefore this proves the truth of his theory. This condition of matters is just as consistent with my theory as with his. When we consider the immense quantity of warm surface-water which, as has been proved, is being constantly transferred from the South into the North Atlantic (a quantity which to a large extent is compensated by a cold current from the Antarctic regions), we readily understand how the polar water comes nearer to the surface in the former ocean than in the latter. In fact, the whole of the phenomena are just as easily explained upon the principle of under-currents as upon Dr. Carpenter's theory.

Dr. Carpenter lays considerable stress on the important fact established by the 'Challenger' expedition, that the great depths of the sea in equatorial regions are occupied by ice-cold water, while the portion heated by the sun's rays is simply a thin stratum at the surface. It seems to me that it would be difficult to find a fact more hostile to his theory than this. Were it not for this upper stratum of heated water there would be no difference between the equatorial and polar columns, and consequently nothing to produce motion. But the thinner this stratum is the less is the difference, and the less there is to produce motion. I have been favored by the Hydrographer to the Admiralty with a series of temperature-soundings taken along the equator; and from these I find that to so small a depth does the super-heating extend, that the surface of the ocean at the equator requires to stand only four and a half feet above that at the poles in order to the ocean being in perfect equilibrium. In this case, if we suppose, in order to constant circulation, that the polar column is kept in ex ess of the equatorial by the weight of, say, two feet of water, there would then remain only a slope of two and a half feet between the equator and poles.—*Phil. Mag.*, June, 1874.

3. *On the Physical Geography of, and the Distribution of Terrestrial Mollusca in, the Bahama Islands;* by THOMAS BLAND. (Ann. Lyc. Nat. Hist., N. Y., x, Nos. 10, 11, March–June, 1873.) —Mr. Bland, whose explorations in West India conchology have made him thoroughly acquainted with the geographical distribution of the species, has brought forward in this paper (which, but for a pressure of original communications, would have been before this cited entire in this Journal) many facts of great interest on the relations of the Bahamas to the adjoining islands. The number of land shells known to exist in the Bahamas is 80, 60 of them inoperculates. He shows that the alliance of the Great Bank with Cuba is very close, as is apparent in the numerous representatives of *Polymita* and *Strophia*, the occurrence of *Polygyra*, *Thelidomus*, and *Melaniella* (three groups unknown in Haiti), and other facts; "while the development of *Plagioptycha* in the Turk Islands and Great Inagua, with the fact that *P. Albertsiana* and *P. disculus* are common to them and Haiti, appears to indicate their connection with the latter island."

After alluding to some observations in Dana's Corals and Coral Islands on the diminished size of the islands to the eastward, and the evidence of subsidence found in the fact, Mr. Bland continues his paper as follows:

"The facts regarding the diminution in size of the islands of the West Indies to the eastward, are of peculiar interest, not only as affording conclusive evidence of the greater subsidence in that direction, but in connection with geographical distribution.

"The banks and islands forming the long Bahama chain diminish in size to the southeast, where are situated at its termination the submerged Mouchoir Carré, Silver and Navidad Banks. In a similar manner, the submerged Virgin Island Bank (with Anegada on its northeastern extremity, geologically, in the opinion of Dr. Cleve, resembling the Bahamas), Sombrero and the Anguilla Bank, terminate the chain of the West Indies (parallel with the Bahamas) eastward from Cuba.

"In the caves of Anguilla, the remains of large extinct mammalia are found, which must have inhabited a far more extensive area, subsequently broken up by subsidence.

"Packard (Amer. Nat., 1872) remarks, 'There is every probability that the separation of these islands (of the eastern part of the West Indies) took place at a late period of time, and probably subsequent to the spread of the Post-pliocene fauna over North America.'*

"Dr. Cleve (*l. c.*) observes that 'the Bahama Islands, the Island of Anegada, and a part of Barbuda, belong to a very recent period.'†

"The same author (*l. c.* 18), referring to the 'Leeward Islands,' states as follows:

* See also Cope, Proc. Acad. Nat. Sci. Philad., 1868, and Bland, Proc. Amer. Phil. Soc., 1871.

† *Helicina convexa* is common to Bermuda and Barbuda.

'The islands north of Guadaloupe form two parallel chains, from northwest to southeast. The western chain commences with *Saba*, and consists of *St. Eustatius*, *St. Kitts*, *Nevis*, *Redonda*, and *Montserrat*. All of those islands are volcanos; and, if the line were extended farther to the north, it would reach the island of Anegada, of Post-pliocene date, and all the volcanos seem to be of the same or nearly the same geological time. The Bahama Islands, which are also most probably of Post-pliocene date, have the same direction, and seem to be the continuation of the same or of a parallel line of elevation. East of the volcanic range is another completely different range of islands. They are not volcanic, and commence with *Sombrero*, comprising *Anguilla*, *St. Martin*, *St. Bartholomew*, *Barbuda* and *Antigua*. All of these islands are of the Tertiary age, *Eocene*, *Miocene*, and *Pliocene*.'

"In his 'Summary of the Geology of the West Indies' (*l. c.* 47), Dr. Cleve says:

'From the facts exposed above, it may consequently be inferred that, of the two prevailing lines of elevation in the West Indies, the one running from west to east originated before the Miocene time, and that the other from northwest to southeast, commencing with the Bahamas and continuing in the same direction down to Trinidad, was formed after the Miocene time.'

"While considering the facts and geological grouping of the islands quoted above from Dr. Cleve's paper, it should be remembered that the land-shell fauna of Saba, of St. Eustatius, St. Kitts, and Nevis (all three on one bank), and of Redonda and Montserrat, and of Barbuda and Antigua (the last two on the same bank) is, in common with most of the islands to the south, to and inclusive of Trinidad, distinct from the fauna of the islands between and inclusive of the Bahamas and Cuba, and the Anguilla Bank, on which are Anguilla, St. Martin, and St. Bartholomew.*

"This difference of the faunas, and the well-defined line of their separation, must be considered in connection with the past and present geological history of the islands.

"The distribution of the species of the genera *Macroceramus* and *Strophia* illustrates in a marked manner the distinctness of the two faunas just mentioned. *Macroceramus* has two species in the Bahamas (one common to the Great Bank, Florida and Cuba, *M. Gossei*, being the only species found in Jamaica); 36 in Cuba, and 10 in Haiti, of which 1 (*M. Gundlachi*) occurs in both.

"There are two other species only in the islands between and inclusive of Porto Rico and those of the Anguilla Bank, *M. signatus*, which, besides Haiti, is found in Tortola, Necker Island, and Anegada, all on the Virgin Bank, and in Anguilla and St. Bartholomew on the Anguilla Bank; *M. microdon* occurs in Porto Rico, Vieque, St. Thomas, Tortola, and Anegada. The genus is not represented in St. Croix, and not in any of the islands south of the Anguilla Bank.

"*Strophia* has 16–18 species in the Bahamas, of which 1 is also in the Florida Keys, and at least 6 in Cuba; 17 in Cuba; none in Jamaica; 2 in Haiti, of which one, *S. striatella*, occurs in Cuba, Porto Rico, Necker Island, and Anegada, and the other, *S. microstoma*, is found also in Cuba, Haiti, and Porto Rico (*fide* Pfr.). Remains of a fossil species, undeterminable, are noticed in Som-

* See Bland, Proc. Amer. Phil. Soc., *l. c.*

brero, and a fossil species in St. Croix. There is no representative of the genus on the Anguilla Bank or to the south of it.

"The exceptions are curious. *Macroceramus Gossei* and *Strophia uva* are found in Curaçao!

"Dana, as already quoted, refers to parallel bands of greater and less subsidence in the Pacific Ocean, and to analogous conditions in the Atlantic;—the subsidence was probably, he says, 'much greater between Florida and Cuba than in the Peninsula of Florida itself; and greater along the Caribbean Sea parallel with Cuba, as well as along the Bahama reefs, than in Cuba.' Recent soundings show in these respects the following facts:

"The greatest depth in the Gulf of Florida, between Key West and Havana, is within five miles of the latter, 800 fathoms (4,800 feet), and I have already stated that there is a depth in the Nicholas Channel, between Salt Key Bank and Cuba, of 534 fathoms (3,204 feet).

"Between Cuba and the east end of Jamaica the depth is 1,244 fathoms (7,464 feet). Eastward of Jamaica, along the southern side of Haiti, in about the latitude of Beata Island, great depths have been ascertained,—one sounding west of that island gave 2,136 fathoms (12,816 feet), and one to the eastward of it 1,840 fathoms (11,040 feet). The greater subsidence still further to the east, between the Virgin Bank and St. Croix, may be inferred from the enormous depth there found, of no less than 2,580 fathoms (15,480 feet).

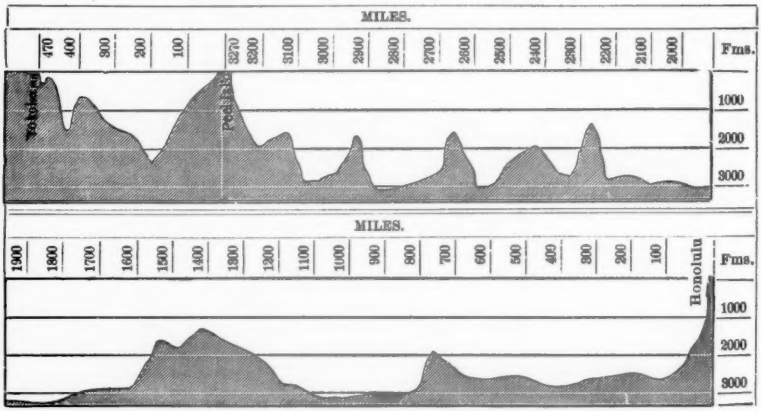
"A line of soundings from the south side of Jamaica and east of the Pedro Bank, across the Carribean Sea to Aspinwall (a distance of about 550 miles), shows the instructive fact that, with no very considerable exception, the sea bottom slopes gradually from Jamaica toward the coast of the Isthmus of Panama. About 60 miles from Manzanilla Point (N. E. of Aspinwall), the depth is 1,215 fathoms (7,290 feet). The bottom then rises comparatively rapidly,—the depth at about forty miles from Aspinwall being 677 fathoms (4,062 feet), and at about twenty miles, 227 fathoms (1,362 feet).

"In connection with the relations of the land-shell faunas of the islands on the north side of the Carribean Sea, I may mention that the greatest depth between the coast of Yucatan and Cape San Antonio, the western extremity of Cuba, about midway between the two, is 1,164 fathoms (6,984 feet): between the east end of Jamaica and the west end of Haiti (so far as is yet known), 600 fathoms (3,600 feet); and north of Mona Island, in the Mona Passage (between Haiti and Porto Rico), 250 fathoms (1,500 feet). I postpone comparison of the faunas of the islands and the adjacent parts of the North American continent; but, in regard to the depth between Haiti and Jamaica on the west side, and Porto Rico on the east, it is noticeable that, while the fauna of Haiti has very little relation with that of Jamaica, it has much alliance with that of Porto Rico."

4. *On the Ocean's bed between Honolulu and Yokohama, from soundings on board the United States ship Tuscarora.*—Sixty casts were taken at intervals averaging about 57 miles. The water fell rapidly and steadily from Honolulu until lat. 21° N., long. 159° 20′ W. was reached (a distance of about 95 miles), to 2,418 fathoms' depth, making a slope of nearly 162 feet to the mile. From that point to lat. 20° 16′ N., long. 168° W., in a distance of 530 miles, there was a slight gradual slope of only four feet to the mile. Between the last named point and lat. 20° 52′ N., long. 172° 39′ W., a distance of 185 miles, there is a submarine mountain, with its summit in about lat. 20° 41′ N., long. 171° 33′ W. Its height is 5,160 feet. It has a slope of 40 feet to the mile on its eastern side, and 128 feet on its western.

From the last station mentioned, where there was 3,045 fathoms of water, the bottom was regular for 240 miles, until lat. 21° 29′ N., long. 178° 15′ W. was reached. Between this station and lat. 22° 1′ N., long. 173° 43′ E., the second submarine mountain was passed, with its summit in lat. 21° 41′ N., long. 176° 54′ E.; its eastern slope averages 37 feet to the mile for about 127 miles from its base, and 51 feet the rest of the distance to the summit. Its western slope is 55 feet; its height is about 12,000 feet.

Diagram of Ocean Bed from Yokohama to Honolulu.



From this last station the plateau can be regarded as level, for over 470 miles, until lat. 23° 31′ N., long. 161° 57′ E. was reached. Between this point and lat. 24° 07′ N., long. 160° 09′ E., the third submarine ridge or mountain was discovered, with its summit in lat. 23° 45′ N., long. 160° 56′ E. Its height is 9,600 feet. Admitting the slope of this mountain to be regular from its base to its summit (which is of course the minimum slope possible), it will have an eastern slope of 192 feet to the mile, and a slope on its western side of 204 feet. Between the last position and lat. 23°

56′ N., long. 156° 10′ E., was the fourth elevation, having its summit in lat. 23° 55′ N., long. 158° 07′ E. From its summit, for a distance of about 45 miles to the westward, there was but a slight inappreciable fall, and from that point to the base a slope of 90 feet to the mile; its eastern slope was 60 feet. Its height is 6,000 feet. For the next 60 miles, the bottom was regular till lat. 24° 02′ N., long. 155° 08′ was reached, between which and lat. 20° 25′ N., long. 153° 01′ was a fifth mountain, extending to the surface in an island known as Marcus Island. A cast was taken in lat. 24° 20′ N., long. 154° 06′ E., a distance of about seven miles from this island, to the northward: 1,500 fathoms of water were found, which gives the land a northern slope to this point of 1,284 feet to the mile. From this point to the eastern base, the slope was about 200 feet to the mile, and to the western, 157 feet.

For 176 miles from the last position, the bottom was, comparatively speaking, regular; then the next 180 miles was occupied by the sixth mountain ridge, its eastern base in lat. 25° 11′ N., long. 149° 46′ E.; its western in lat. 26° 09′ N., long. 146° 10′ E., and its summit in lat. 25° 42′ N., long. 148° 39′ E. Its height was about 7,800 feet; its eastern slope, 163 feet to the mile, and its western 59 feet. From this point to Port Lloyd, a distance of 210 miles, the slope was about 86 feet to the mile. I have taken, in consideration of the height and slope of the submarine elevation, about 3,000 fathoms of water as the depth of the plateau proper; for it was only at that distance that it remained regular for any distance, and again the average depth between the bases of the mountains or mountain ridges, and for over half the entire distance of the line, was about that. All the slopes computed are the minimum.—*N. Y. Tribune*, June 13.

5. *Meeting of the American Association at Hartford.*—The twenty-third meeting of the American Association, at Hartford, commenced on Wednesday, the 12th of August, and closed on the following Wednesday. Dr. John L. LeConte, of Philadelphia, was the President of the meeting, and Professor C. S. Lyman, of New Haven, the Vice-President. The attendance was large, the papers numerous and for the most part excellent, and the interest in the sessions was sustained to the end, although the meeting was prolonged one day beyond the usual time. The citizens of Hartford contributed very largely to the pleasure and scientific profit of the occasion, through the many excursions for which they had made liberal arrangements. These included a trip by steamboat down the Connecticut to its mouth; and others—to the Rocky Hill quarry, where the trap and its junction with the sandstone is well exposed; the silk manufactory at Cheneyville; Tarriffville and its gorge through the trap and sandstone; the Portland quarries, opposite Middletown, where, in the bottom of one quarry, *twenty-one consecutive tracks of the huge Otozoum Moodii, along a line over fifty feet long*, were exposed, having been recently opened to view; and on Thursday, the day after the close of the session, by rail, to Salisbury, a region of extensive limonite exca-

vations, 62 miles west of Hartford. The address of the retiring President, Professor Lovering, of Cambridge, was delivered on Friday evening; it was an admirable review of the recent progress of physical science. The next meeting will be held at Detroit, Michigan, during the week commencing with Wednesday, the 11th of August. J. E. Hilgard, of the Coast Survey, was appointed President of the meeting.

The following is a list of the papers accepted for reading:

Section A.

Differential Measurements of Solar Temperature; S. P. LANGLEY.

Velocity of Primitive Undulations; P. E. CHASE.

The Distribution of the Poles of certain Nebulæ; C. ABBE.

Exhibition of a Combined Collimator and Personal Equation Machine, with a Notice of the Results obtained with it; W. A. ROGERS.

Sudden Fluctuations of Levels in quiet waters:—Records of Observations; C. WHITTLESEY.

Relation between the Barometric Gradient and the Velocity of the Wind; W. FERREL.

New way of illustrating the vibrations of organ-pipes; J LOVERING.

On a rotating, terrestrial Planisphere; S. D. TILLMAN.

The Phantascope or Zoötrope as a means of illustrating crystallography to a class; R. H. RICHARDS.

Notice of Prof. A. K. Eaton's new compound one-prism Spectroscope; R. E. ROGERS.

On the number and distribution of the brighter fixed stars; B. A. GOULD.

On the proper motion of Eta-Draconis in Right Ascension; W. A. ROGERS.

On the Harvard College Observatory System of Communicating Time for Civil Purposes; W. A. ROGERS.

Description of a new Mechanism for printing hourly the Directions and Velocity of the Winds; G. W. HOUGH.

On a method of transmitting Time Signals over Telegraph wires; G. W. HOUGH.

On the Influence that varying Atmospheric conditions have upon the perception of Sounds, especially those of the human ear, with experiments; L. TURNBULL.

A new demonstration of an old theorem in Geology; B. S. HEDRICK.

On a simple form of Sphygmograph; A. A. BRENEMAN.

On an improved Electric Bell; A. A. BRENEMAN.

Simple Formulas for some of the Higher Curves; J. D. WARNER.

On the Mineral Warwickite; J. L. SMITH.

On Metallic Iron in Basaltic Rock and the dissociation of Oxide of Iron; J. L. SMITH.

Curious Association of Datolite, Garnet, and Idocrase; J. L. SMITH.

A Convenient Method of Making Absolute Alcohol; J. L. SMITH.

Exhibition of a new Sulphuretted Hydrogen Apparatus; H. CARMICHAEL.

A new Method for the Illustration of Sound Waves; H. CARMICHAEL.

Exhibition of a Platinum Digestor; H. CARMICHAEL.

On the Phosphorescence of Glass, produced by electricity; A. W. WRIGHT.

On the use of Natural Twin Crystals of Quartz in the construction of Polariscopes; A. W. WRIGHT.

On the nature of the Zodiacal Light, and the distribution of the matter which occasions it; A. W. WRIGHT.

On the Tails of Comets; H. M. PARKHURST.

Loss of Light in its transmission through Space; H. M. PARKHURST.

On the Pressure produced by Impact; J. J. SKINNER.

Some Improvement on Descartes' Barometer; B. S. HEDRICK.

On Abnormal Phenomena of Sound; J. HENRY.

On the Estimation of Nitrogen by the absolute method; S. W. JOHNSON.

Best methods of making pure Carbonate of Soda and Carbonate of Potash for analysis; J. L. SMITH.

Report on a remarkable compound of Phosphorus and Carbon, forming a new Pyrophorus; P. H. VAN DER WEYDE.

On the Estimation of Nitric Acid by the methods of Thorpe and Bunsen; S. W. JOHNSON.

Investigation into the chemical nature of Petroleum; P. H. VAN DER WEYDE.

On the alleged formation of Ammonium Nitrite from Water-Vapor and Nitrogen, and on Price's Test; S. W. JOHNSON.

The change of Peat into Cannel or other Coal by Heat and Pressure, practically demonstrated; P. H. VAN DER WEYDE.

On the Thermo-electrical Properties of some Minerals and their Varieties; A. SCHRAUF and E. S. DANA.

The Nitrogen of the Soil; H. P. ARMSBY.

On the Periodicity of the Rainfall in the United States in relation to the Periodicity of the Solar Spots; J. BROCKLESBY.

An account of a Remarkable Phenomenon connected with the quarrying of marble in Rutland, Vermont; J. BROCKLESBY.

On the molecular volume of water of Crystallization; F. W. CLARKE.

On the molecular heat of similar compounds; F. W. CLARKE.

Action of Mechanical Vibration in retarding Chemical Combination; S. S. HALDEMAN.

Sewage Question chemically considered; T. S. HUNT.

On Wet Processes of Copper Extraction; T. S. HUNT.

On the Cement of Ransome's new Artificial Stone; T. S. HUNT.

On Lithium Glass; C. B. DUDLEY.

On the use of a glazed wrought iron tube for Nitrogen Determinations; A. P. S. STUART.

The Chemistry of Steel; B. S. HEDRICK.

Outlines of the Typical Photo-lithographic Processes; J. W. OSBORN.

On a Modification of Loewenthal's Method for the Estimation of Tannic Acid; W. MCMURTRIE.

The Inner Satellites of Uranus; E. S. HOLDEN.

On a new form of Steam Generator, applicable especially to very small powers; W. P. TROWBRIDGE.

Annual Mortality of Officers of the U. S. Army for fifty years, from 1824 to 1873, including deaths in war; B. ALVORD.

Mutual Action of Elements of Electric Currents; E. B. ELLIOTT.

Periodicity of Rates of Interest from observations for the 25 years 1849 to 1874; E. B. ELLIOTT.

Population of the United States, each year, from 1780 to 1880, with proof of estimate and interpolation; E. B. ELLIOTT.

Credit of the United States Government; E. B. ELLIOTT.

Expenditures of the United States Government per capita to population, by four year's periods from the organization of the Government, to the 30th of June, 1874; E. B. ELLIOTT.

Section B.

On the Cave Fauna of the Middle States; A. S. PACKARD, Jr.

Change by Gradual Modification not the Universal Law; T. MEEHAN.

On the Insects more particularly associated with *Sarracenia variolaris* (Spotted Trumpet leaf); C. V. RILEY.

On the Summer Dormancy of the Larvæ of *Phycoides nycteis* Doubleday, with Remarks on the Natural History of the Species; C. V. RILEY.

On the Habits and Transformations of *Canthon hudsonias* (Forst.)—the common "Tumble-dung;" C. V. RILEY.

On the Larval Habits of the Cantharid genera *Epicauta* and *Henous;* C. V. RILEY.

On the Origin of North American Unionidæ; E. S. MORSE.

On the Relations of Dentalium; E. S. MORSE.

On the ascending process of the Astragalus in Birds; E. S. MORSE.

The Lobster; W. W. WHEILDON.

Glacial Phenomena in the Sierra Nevada; J. MUIR.

Note on the gestation of the little Brown Bat; B. G. WILDER.

Botanical Observations; W. H. SEAMAN.

On an Organ of Special Sense in the Lamellibranchiate genus Yoldia; W. A. BROOKS.

The Wings of Pterodactyls; O. C. MARSH.

Small Size of the Brain in Tertiary Mammals; O. C. MARSH.

On the Male and Female Organs of the Sharks, with special reference to the use of the "Claspers;" F. W. PUTNAM & S. W. GARMON.

The Genera of Butterflies studied Historically; S. H. SCUDDER.

The Recency of certain Volcanoes of the Western U. S.; G. K. GILBERT.

Notes on Natural Erosion by Sand in the Western Territories; G. K. GILBERT.

The advantages of the Colorado Plateau Region as a field for geological studies; G. K. GILBERT.

On the Disintegration of Rocks and its Geological Significance; T. S. HUNT.

Growth of Crystallization in traps and slags; H. CARMICHAEL.

Some remarks on certain German trap rocks; H. CARMICHAEL.

On *Sarracenia variolaris* as a Fly Catcher; J. H. MELLICHAMP.

Darlingtonia Californica an insectivorous Plant; W. M. CANBY.

On Regeneration or Organic Molecular Conservation: a contribution to the doctrine of evolution; L. ELSBERG.

On the Cotton Worm (*Aletia argillacea* Hübn.); A. R. GROTE.

Physical History of New Hampshire; C. H. HITCHCOCK.

Geological Map of the United States and Territories, with Critical and Explanatory Descriptions; Prof. C. H. HITCHCOCK and W. P. BLAKE.

Discovery of twelve skeletons of *Dicotyles compressus* in the Valley Drift in Columbus, Ohio; J. H. KLIPPART.

On the organic Change produced in the Bee by the different conditions to which it is subjected in its larval state; Mrs. S. B. HERRICK.

A list of the Vertebrate Animals of Outagamie Co., Wisconsin, with notes, etc.; D. S. JORDAN.

Notes on some rare and interesting Carices of New York; G. VASEY.

Are there plants the seeds of which before germination lie in the ground a definite number of years; J. HYATT.

Notice of a pair of Trap-door Spiders from South Carolina; C. R. DODGE.

How do young Birds peck out of the shell; J. W. P. JENKS

An Inquiry concerning the Reversion of Thoroughbred Animals; W. H. BREWER.

On the present Distribution of Woodlands within the United States; W. H. BREWER.

Notes on Tree Growth; A. GRAY.

On the Classification of the Indian Languages of Mexico; P. C. BLISS.

Remarks on the Anderson School of Natural History on Penikese Island; F. W. PUTNAM.

An instance of Replacement of Injurious Insects by Human Agency; J. L. LE CONTE.

On the Equivalency of the Coal-measures of the United States and Europe; C. A. WHITE.

On some localities of contact of Trap and Sandstone in the Connecticut Valley; W. N. RICE.

Further observations on the Geology of Northwestern Massachusetts, with special reference to the Hoosac range; S. TENNEY.

On the composition of the Pottery of the Mound-builders; E. T. COX.

On the true character of the so-called Eozoon Canadense; L. S. BURBANK.

On the Mechanical condition of the Pebbles in the Newport Conglomerate and their supposed flattening by pressure; W. B. ROGERS.

On the thickness of the Virginia Tertiary, as indicated by the Artesian borings at Fortress Monroe; W. B. ROGERS.

Notes on the Paleozoic Formations of South America; O. A. DERBY.

On the Trap rocks of the Connecticut Valley; E. S. DANA.

The Physical and Geological Characteristics of the Great Dismal Swamp and the Eastern Counties of Virginia; N. B. WEBSTER.
Origin of the Cascades of the Columbia River, Oregon; W. P. BLAKE.
Observations in a visit to the Cave of Cacahuamilpa, in Mexico; P. C. BLISS.
An ascent of the Volcano of Popocatapetl in Mexico; P. C. BLISS.
Observations on the Mesozoic of North Carolina; W. C. KERR.
On the Significance of Classes among Vertebrates; T. GILL.
On the Relations of certain Genera of Cervidæ; T. GILL.
Progress of Science in Maryland; Mrs. A. L. PHELPS.
Cremation among North American Indians; J. L. LECONTE.
A Remarkable Ancient Stone Fortification in Clark County, Ind.; E. T. COX.
Remains of an Ancient Earth Work in Marblehead, Massachusetts; J. J. H. GREGORY.

6. *Chemical Centennial.*—The first day of August, 1774, illustrious as the date of Joseph Priestley's discovery of oxygen, was commemorated on the first of August, 1874, by the gathering of American chemists at Priestley's grave, on the banks of the Susquehanna, at Northumberland, Pennsylvania, and by a like gathering of British chemists at Birmingham, England, for the unveiling of a statue to Priestley. Both gatherings appear to have been prosperous, and the interchange of salutations by ocean cable placed the two assemblies in sympathy on an occasion well calculated to awaken kindred sentiments. The American gathering was presided over by Professor Charles F. Chandler of the Columbia College School of Mines, who responded to an address of welcome from the citizens of Northumberland, delivered by Col. David Taggart of that place.

The following addresses were also delivered on the occasion, by previous appointment:—

A Sketch of the Life and Labors of Dr. Joseph Priestley by Professor Henry H. Croft, of Toronto, Canada.

A Review of the Century's Progress in Theoretical Chemistry, by Professor T. Sterry Hunt, of Boston, Massachusetts.

A Review of the Century's Progress in Industrial Chemistry, by Professor J. Lawrence Smith, of Louisville, Kentucky.

An Essay on American Contributions to Chemistry, by Professor Benjamin Silliman, of New Haven, Connecticut.

Professor Joseph Henry of Washington, who had been expected to preside at the meeting, and to deliver an address at Priestley's grave, was unfortunately detained by a sudden illness, and in his absence the assembly, gathered at sunset at the grave of Priestley, on Friday evening, July 31st, was addressed in a most appropriate manner, by President Henry Coppée, of Lehigh University.

Many descendents of Dr. Priestley reside at Northumberland. Dr. Joseph Priestley was the chairman of the local Committee of Arrangements; and another of the same degree of consanguinity, the third generation, was among those whose hospitalities added to the pleasures of the occasion. The old Priestley mansion, with its ample apartments, was thrown open to the guests, and in one of them was a collection of apparatus and other personal memorials of the eminent man whose name consecrated the day and the place.

The telegraphic dispatches above alluded to were as follows:

The American Chemists assembled at Northumberland, Penn.: Our marble statue representing Priestley discovering oxygen will be unvailed to-morrow, presented by the subscribers through Professor Huxley to the town, and accepted by the Mayor. We greet you as colleagues in honoring the memory of a great and good man. THE PRIESTLEY MEMORIAL COMMITTEE,
Birmingham, Eng.

NORTHUMBERLAND, Penn., July 31, 1874.
The brother chemists at the grave to their brothers at the home of Priestley send greeting, on this centennial anniversary of the birth of chemistry.

We understand that the addresses will appear in a Memorial number of the American Chemist, soon to be published, which will contain also a full account of the proceedings. B. S.

7. *Elements of Metallurgy*, &c.; by J. ARTHUR PHILLIPS, M.Ins.C.E., F.G.S. 764 pp. 8vo, with 205 illustrations on wood. London, 1874. (Chas. Griffin & Company.)—This new edition of Mr. Phillips's well known "Manual of Metallurgy" (1852, 1854 and 1858) bears date June, 1874. It is in fact a new book. About one-half of the volume (354 pages) is given to the subjects of Fuel (106 pages), and Iron and Steel (248 pages). It is remarkable for its clear, concise and comprehensive style, by which qualities a vast amount of valuable and accurate information is brought within a moderate compass. The author's own practical skill and experience, both in metallurgical operations and in personal explorations of many of the metalliferous regions and workings described, add to the value of his work. It is especially interesting to American readers from its frequent citations of American examples. Some omissions we note: for example, under the "Wet Processes for Copper," no mention is made of the Hunt and Douglass process, depending on the solution of oxide of copper in a bath of sodic chloride and ferrous chloride forming soluble chloride and dichloride of copper, from which metallic iron throws down the metallic copper, regenerating the ferrous chloride which had been consumed in chloridizing the copper oxide and thus renewing the bath for a fresh attack. This very ingenious and original process is now in successful use here on a large scale and is certainly worthy of mention in so good a book as Mr. Phillips's Metallurgy. B. S.

8. *Volume of Collected Researches of J. Lawrence Smith*, 1874.—A letter received by us from Dr. Smith states that the apparent injustice to Professor Brush, referred to in the August number of this Journal, page 144 of this volume, was wholly unintentional on his part, and desires us to request that those having the work should change the wording of the note (p. 109) by substituting "associated with" for "assisted by."—EDS.

On the Marine Mammals of the North Pacific, by Capt. C. M. Scammon, U. S. Revenue Service. 4to, with many plates. San Francisco, 1874. (John H. Carmany & Co.)

My Visit to the Sun; or Critical Essays on Physics, Metaphysics, and Ethics. By Lawrence Benson. Vol. I. Physics. 158 pp. 8vo. New York, 1874. (James S. Burton.)